

THE PSYCHOLOGICAL REVIEW.

PROCEEDINGS OF THE FIFTH ANNUAL MEETING
OF THE AMERICAN PSYCHOLOGICAL ASSO-
CIATION, BOSTON, DECEMBER, 1896.

REPORT OF THE SECRETARY AND TREASURER FOR 1896.

The fifth annual meeting of the American Psychological Association was held in Boston and Cambridge, December 29 and 30, 1896, the time and place having been chosen with reference to the simultaneous meetings of the American Society of Naturalists and the Affiliated Societies. There were forty-five members in attendance, the largest number since the organization of the Association. Three formal sessions were held, one on the morning of the 29th at the Harvard Medical School in Boston, and two sessions on the 30th at the Peabody Museum of Archæology in Cambridge. The morning session of the 30th was given up to papers of a distinctly philosophical character. The members of the Association for the most part attended the discussion on 'The Inheritance of Acquired Characteristics' before the American Society of Naturalists on the afternoon of the 29th, psychology being represented in the discussion by Professor James, of Harvard. Together with the other Affiliated Societies, the psychologists were present at Mr. Alexander Agassiz's lecture and reception in the evening of the 29th, at the luncheon given by the President and Fellows of Harvard College on the 30th, and at the annual dinner of the Societies at the Hotel Brunswick, Boston, on the evening of the same day. President George S. Fullerton presided at all the meetings of the Association.

At the regular business meeting and in the intervals of the program the following business was transacted: Election of officers for 1897: *President*, Professor J. Mark Baldwin, of Princeton University; *Secretary and Treasurer*, Dr. Livingston Farrand, of Columbia University; *Members of the Council*, Professors Josiah Royce, of Harvard University, and Joseph Jastrow, of the University of Wisconsin. Elected to membership on nomination of the Council: Dr. Ernest Albee, Cornell University; Dr. C. F. Bakewell, Harvard University; Dr. E. F. Buchner, Yale University; Mr. A. F. Buck, Union College; Mr. J. F. Crawford, Princeton University; Professor F. C. French, Vassar College; Dr. Alice J. Hamlin, Mt. Holyoke College; Professor J. G. Hibben, Princeton University; Dr. C. W. Hodge, Princeton University; Dr. David Irons, University of Vermont; Professor R. B. Johnson, Miami University; Dr. C. H. Judd, Wesleyan University; Dr. Robert McDougall, Western Reserve University; Professor G. H. Palmer, Harvard University; Mr. F. C. S. Schiller, Cornell University; Dr. G. A. Tawney, Beloit College.

An invitation was received from the British Association for the Advancement of Science to attend the next annual meeting to be held in Toronto, Canada, as members of the Section of Physiology. It was moved and carried that such members of the Council, including the outgoing members, as are able to attend, be official delegates of the Association to that meeting, and that such members of the Association as may be able to do so accept the invitation to attend as members.

An invitation was received from the American Association for the Advancement of Science to join that Association. It was voted that all members who might feel so disposed are recommended to present their names to the Secretary for election to that Association.

A communication was received from the Director of the Biological Laboratory, at Wood's Holl, inviting the Association to hold an informal meeting at Wood's Holl during some week of the summer months.

The invitation was referred to the Council, which recommended that the question of an informal Summer meeting of the As-

sociation be favorably considered, and that Wood's Holl be regarded as an eligible place for such meeting.

The following motions were made by Professor Witmer, and were referred to the Council :

1. That the Council of the American Psychological Association be recommended to select only such papers and contributions to the program of the annual meeting as are psychological in subject-matter.

2. That the Council of the American Psychological Association be recommended to present at the next meeting of the Association a plan for the formation of an American Philosophical or Metaphysical Association, as one of the affiliated or associated organizations meeting with the present Affiliated Societies.

3. That in the election of new members to the American Psychological Association all names nominated by the Council shall be presented to the Association at its opening session in written form, or visibly displayed upon a blackboard, together with a statement of the contribution or contributions to psychology, in virtue of which the persons named are eligible to membership, and that the action upon such names shall be taken by the Association at the final business meeting.

The time and place of the next meeting were referred to the President, to be determined in consultation with the authorities of the Affiliated Societies.

The Committee on Physical and Mental Tests presented their report, and, after a vote of thanks for the hospitality shown by Harvard University and the Local Committee of Arrangements, the meeting adjourned.

The report of the Treasurer and the abstracts of papers read at the meeting follow :

REPORT OF THE TREASURER :

Livingston Farrand in account with Am. Psychological Association.

DR.

To receipts from retiring Treasurer.....	\$308 09
“ Dues of Members.....	177 00
“ Estimated Interest on Deposits.....	15 00
	<hr/> \$500 09

Cr. By Expenditures for	
Postage and Stationary.....	\$11 90
Printing and Clerical Work.....	14 25
Expressage	40
	<hr/>
	\$ 26 55
Balance on hand.....	\$473 54
Audited by the Council and found correct.	

LIVINGSTON FARRAND,
Secretary and Treasurer.

ABSTRACTS OF PAPERS.

The Physiology of Sensation. By E. A. SINGER, University of Pennsylvania.

States the fundamental question as: What would be an ideally complete physiology of sensation? The method employed in answering the question would establish an analogy between what has been regarded as progress in the past and what should be sought by a progressive psychology of the future. All the validity claimed for the method rests upon our right to speculate until facts be forthcoming. The result of such an analogy is stated in the following form: wherever we know anything about the physiology of sensation, we find that the correlate of a mental difference is a structural physiological difference. Where we are yet in ignorance as to the physiological counterpart of a mental difference we should assume it to be a difference in structure rather than a difference in functioning of the same structure. This view is to be contrasted with such current opinions as would regard the physiological counterpart of intensity as the greater or less activity of the same nervous structure; feeling tone as the greater or less disintegration, or as dependent upon conditions of greater or less nutrition of the same structure, etc. Some attempt is made, rather by way of illustration than as framing a completely tenable hypothesis, to suggest a physiology of these so-called properties of sensation that would relate them to quality of sensation. Thus the physiological basis of intensity differences is sought in part in the different end organs affected in

greater or less reaction to a stimulus; in part also in special apparatus suggested by the allied nature of intensity and saturation in color sensations. Feeling tone is distinguished from pleasure and pain; the physiology of the former being related to that of the emotion, the physiology of the latter to that of the special senses. Local sign presents the inverse problem as to how sensations conditioned by confessedly different nervous structures should come to be classed together. The answer suggested is that the classing together of locally different sensations and qualitatively similar is conditioned by the formal likeness of the end organs affected, they determining a likeness in the adequate stimuli and in the general way of behaving of the sensation. Recognized likeness and difference of sensations are found to involve psycho-physical reflection.

Intensity of Sensation. By JAMES E. LOUGH, Harvard University.

Sensations forming an intensity series have this characteristic which distinguishes them from a qualitative series: namely, that the intensity series goes towards or from zero—the vanishing point—while a purely qualitative change leads neither to nor from the zero point of sensation. Theories of intensity of sensation may be classed in general under two heads: (a) that the stronger sensation *is* the weaker sensation plus more of the same sensation—following an analogy from the physical world which may prove dangerous and exposing psychology to the troublesome presupposition that our psychic elements (sensations) are compounds; (b) that the intensity series is merely a qualitative series, but ordered in a series towards or from zero by the presence of a second series of sensations, *e. g.*, brightness sensations or muscular sensations.

It would seem much more satisfactory to discover in the nature of the psycho-physical process itself that which shall give to sensations the characteristic of an intensity series. Accordingly, this hypothesis is offered: any sensation of a given quality and intensity that may arise, depends upon a certain physiological condition which is reached only after passing successively through a series of other physiological conditions, each

of which is the basis of a sensation of the same quality, but differing in degree from zero to the given sensation. That is, any sensation depends upon the physiological basis which contains, in a temporal series, the bases of all the weaker sensations of this particular quality. The final neural condition, after passing through all the intermediate steps, may be called the maximum effect of the stimulus. By a study of the intensity of sensations produced by a stimulus of a known intensity acting for a time less than that necessary to produce its maximum effect, it is found that this intensity is exactly proportional to the duration of the stimulation. Concerning the nature of the psycho-physical process nothing is postulated save that the basis of the stronger sensation contains that of the weaker in the time series as stated above.

Report of Experiments on the Reduction of the Tactual Double-Point Threshold by Practice, and on the 'Vexirfehler.'

By G. A. TAWNEY, Beloit College.

The first object of the following experiments was to examine the view of Volkmann and Fechner that, by daily practicing some one spot of skin in the perception of two points, the threshold for this perception is reduced, not only for the spot actually practiced, but also for the symmetrically opposite spot on the other side of the body. A number of threshold determinations were made on different parts of the body varying in number from six to thirty-two for each subject. One of these spots was chosen for special practice which continued for a period varying from two weeks to a month. At the end of this time, the threshold determinations on the six to thirty-six different parts of the body were repeated, in order to compare them with those at the beginning of the practice series. The instrument used was a simple pair of compasses. The results show unmistakably that where any reduction of the threshold occurs as a result of practice, it occurs over the entire surface of the body; it demands, therefore, a central explanation. The paper further discusses the nature 'Vexirfehler' (double-point illusion). It was assumed that the double-point illusion is the result of suggestion and it was sought to free a subject, whose

threshold formerly could not be determined, from the suggestion involved. The experiments seemed to show that the reduction of the threshold by practice is, to a great extent at least, a result of suggestion. Several series were carried out for the purpose of studying the psychosis underlying the 'Vexirfehler.' The results seem to show that this illusion is mainly due to auto-suggestion, although physiological factors may play a subordinate part.

Comparison of the Times of Simple Reactions and of Free-Arm Movements in Different Classes of Persons. By ALBERT L. LEWIS. (Introduced by Professor Witmer.)

This paper gave the results of nearly 9,000 experiments on American men and women, and on male Negroes and Indians. The relative order of these four classes was found to be in reacting to Sound, arranged from shortest to longest: Indians, American men, Negroes and American women; to Light, American men, Indians, American women and Negroes; to Touch, Indians and American men the same, Negroes third and American women fourth. With regard to the mean variations of the average reaction times, the order was: in Sound, American men, Indians, Negroes and American women; in Light, American men, Indians, Negroes and American women; in Touch, Indians, American men, American women and Negroes. Following this was given a comparison of the flexion and extension movements of both right and left arm. The relative order of the classes was found to be American men, Indians and American women. This order holds throughout the four movements. No report on the Negroes was given, as the experiments on this class were not yet completed. It was noticeable in these experiments that the American men were quicker in their longest movement than the Indians in their shortest, and the Indians were similarly quicker than the women. Not less interesting was the fact that, although all the subjects were right-handed, the flexion movements of the left arm were quicker than the corresponding movement of the right, except in the case of the American men, where the time of the movement was the same for each arm. The conclusions drawn were that there are

characteristic variations in the reaction time and rate of movement of classes of persons; that a close relation exists between reaction time and rate of movement; that a number of reactions is necessary to give a characteristic result in each individual case.

Researches in Progress in the Psychological Laboratory of Columbia University. By J. McKEEN CATTELL.

Among the subjects in course of investigation the following may be mentioned as likely to be completed soon: Mr. W. Lay, lately Fellow in Philosophy, has for several years been studying mental imagery by various methods. In addition to questions such as those proposed by Mr. Galton, others have been set more independent of immediate introspection and extending to auditory and motor imagery. Among others, including musicians, 100 leading artists have in letters and interviews described their imagery. Imagery has been investigated by its effects on memory, and in the compositions of poets and other writers. Mr. Lay has, finally, given special attention to his own imagery and associations. Mr. S. I. Franz, Fellow in Psychology, is investigating after-images. He has already published experiments on the threshold for after-images, and is now studying the duration and nature of the after-image as dependent on the intensity, duration and area of stimulation. He is able to correlate the effects of these magnitudes for consciousness and to analyze physiological and mental factors. The individual differences are of interest, for with the same stimulus the image differs greatly with different persons. Mr. L. B. McWhood, Fellow in Psychology, is studying the motor accompaniments of the perception and emotional results of music. The movements are a series of taps made as rapidly as possible and a pressure, not a maximum but kept as nearly as may be constant. The subject decides on his preferences, etc., for the tunes and combinations used, and these are compared with the motor effects. Mr. H. E. Houston is studying color nomenclature, with special reference to children, and proposes to extend his work to other senses. The growth in accuracy and extent of the color vocabulary in schools has been determined, and the attempt will be made to find and set a normal nomenclature for

colors and other classes of sensations. Other researches were referred to briefly.

The Psychic Development of Young Animals and its Somatic Correlation, with Special Reference to the Brain. By WESLEY MILLS, McGill University, Montreal.

This paper is based on researches on psychic development and on the development of cerebral cortex in the same groups of animals. As somatic correlation other than that of the brain has been considered in other papers, that phase of the subject was not especially treated in this paper. The main conclusions are as follows: in the dog and the cat there is a period extending from birth to about the time of the opening of the eyes characterized by reflex movements, the sway of instincts and the absence of intelligence. During this time the cerebral cortex is inexcitable by electrical stimulation, so that the psychic condition during the blind period is correlated with an undeveloped state of the motor centers of the cortex of the cerebrum. The advance in movements, first of the limbs and later of the head and face parts, together with the psychic progress associated with this, is correlated with the rapid development of the cortical centres for the limbs in the first instance, and later for the head and face in the period immediately following the blind stage. This is more rapid and more pronounced in the cat than in the dog, and is correlated with the greater control in the cat over the fore-limbs and with certain physiological and psychic developments characteristic of the cat.

Similar conclusions apply to the rabbit, except that the difference in the rapidity of development of head and face movements is correlated with an earlier organization of the corresponding cortical centres, and that there is a greater difference between the fore-limb and the hind-limb, with all of which there are special psychic correlations bound up with certain peculiarities of the rabbit's modes of life.

The vast difference in physiological and psychic development of the cavy at birth is correlated with the presence of cortical cerebral centres readily excited by artificial stimuli, centres which in a few days reach a practically perfect state of development.

The psychic manifestations of the pigeon and the fowl have not the same sort of cerebral cortical correlates as the animals referred to above.

The Organization of Practical Work in Psychology. By
LIGHTNER WITMER, University of Pennsylvania.

Under the designation of practical work in psychology was included: 1. The direct application, whether by professional psychologists, practicing physicians or teachers, of psychological principles to therapeutics and to education.

2. Such psychophysical investigation of mental conditions and processes as may serve to throw light upon the problems that confront humanity in the practice of medicine or teaching.

3. The offering of instruction in psychology, to students of medicine or to teachers, that contains a promise of future usefulness to them in their respective professions.

Thus the plan has a view to the professional practice of psychology, to research and to instruction, as these stand related to the two professions of medicine and teaching.

In order that psychology may become a usable possession of the medical man, details of organization must be perfected that will bring about a union of the department of psychology with the professional departments of the medical school.

The following details of organization are suggested as a part of a plan for the development of research work and instruction useful to the community and to the teacher:

1. The University Department of Psychology should be in close association with all classes and grades of children. Organization is required to make possible the conduction of physical and mental tests upon all children in all grades from the kindergarten up to the graduate department and upon such so-called abnormal children as may be found in special institutions for the feeble minded, the deaf, the blind and the morally defective.

2. A Department of Psychology needs for purposes of demonstration a Psychological Museum equipped with specimens of work done by defective and by normal children with the instruments and apparatus used in teaching them, and affording some

display of the results of special investigations into the physical and mental characteristics of children, exhibited, perhaps, in the form of tables and curves.

3. An experimental training school presenting the following features :

a. Independent schools or homes for such children as can afford to pay for expert psychological and pedagogical treatment.

b. A psychological clinic and dispensary.

c. Special or ungraded training schools for children who are backward or physically defective—these to be organized under the control of the city school authorities but to be in harmonious and effective relation with the Psychological Department of the local university.

4. Instruction in psychology should be adapted to meet the wants of two classes of teachers :

a. The common school teacher of all grades from the kindergarten to the university, who needs, above all else, courses in the practical study of children.

b. The psychological expert who is capable of treating the many difficult cases that resist the ordinary methods of the school room. The pedagogical or psychological expert requires thorough courses in some branches of medicine and in practical psychology. For both classes of teachers, the features just enumerated under one, two and three, will be found of great service in supplying the requisite practical experience in psychology.

Psycho-Physical Tests on Normal School and Kindergarten Pupils. By MISS MARY P. HARMON (Introduced by Prof. Witmer).

These tests form part of a general scheme which proposes the development of a series of tests which shall be applicable alike to the oldest and youngest pupils in all grades from the Kindergarten to the Normal School. The intention is to repeat from year to year a series of experiments of which a few are included in this preliminary report as the children now in the Kindergarten pass through the various grades.

The tests reported upon include family statistics, age, height,

weight, lung capacity, simple reaction time to sound and rate of free arm movements.

One hundred girls in the first year of the Normal School were tested. Ninety-five per cent. are American born. Seventy-nine per cent. of the mothers and seventy per cent. of the fathers are American born. The grandparents range from forty-one to forty-four per cent. American born. The average age is 18.2 years. The average height is 62.4 inches, the average weight is 112.8 pounds, and the average lung capacity is 134.4 cubic inches. The average reaction time taking the minimum of five trials is 153 σ . The average quickest movement is for the right hand in extension 114 σ , in flexion 108 σ ; left hand in extension 109 σ ; left hand flexion 217 σ ; the distance moved over was 53 centimeters. Some coördination was noticeable between intellectual capacity and rate of reaction and movement; those girls who stood out as bright being below the average.

Thirty-four boys and sixty-three girls in the Kindergartens ranging from four and one-half to seven years of age give an average height of thirty-nine to forty-four inches. Weight thirty-five to forty pounds. The boys give an average lung capacity of forty cubic inches; the girls of twenty-five. The boys give an average shortest reaction of two hundred and eighty-four, the girls of two hundred and eighty-one. The boys give an average longest reaction of five hundred and thirty-three, the girls of six hundred and eighty-four. The boys move the right arm in extension, the left in flexion, the left in extension and the right in flexion in the following times respectively: 153, 154, 158, 160; the girls make the same movements in the following time: 219, 205, 228, 223. The average shortest time for all movements is 153 for the boys and 192 for the girls. Thus the girls throughout are noticeably slower than the boys, although their reaction time is but a trifle longer. Comparing the Kindergarten children with the Junior Class of the Normal School we find the average shortest reaction of the Juniors is 131 σ shorter than that of the Kindergarten boys and 128 σ shorter than that of the girls. Their rate of movement is 62 σ less than for the boys and 101 σ less than for the girls.

Personal Experiences under Ether. By WESLEY MILLS, McGill University, Montreal.

This paper relates the experiences of the writer during and immediately subsequent to the administration of ether, together with a later experience which seemed to grow out of the former and which produced a profound impression.

A Preliminary Study of Memory. By BROTHER CHRYSOSTOM, Manhattan College.

The paper presented rather a plan of work for the present scholastic year than results already obtained from experiments on memory. The immediate end sought was a knowledge of the relative value of visual and aural memory. The method adopted is similar to that described by Mr. E. A. Kirkpatrick in the *PSYCHOLOGICAL REVIEW*, November, 1894. The two series, viz., of objects for vision, and of names for hearing, are shown on alternate days. Both names and objects are familiar, the names being monosyllables and excluding association effects. The objects and names recalled with greatest facility are then noted and classified with a view to determine the cause of this phenomenon. At this stage the work is again reviewed and examined as helping to answer the question: "Is conscious memory a *spiritual* phenomenon?"

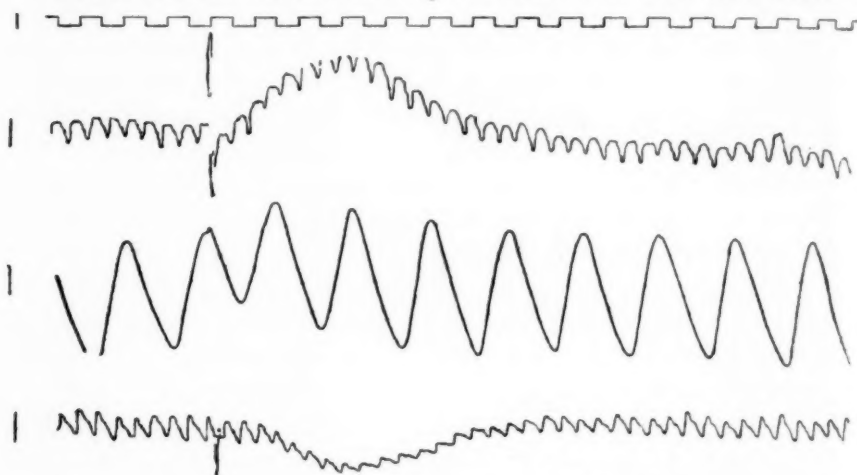
Lest the conditions under which the experiments are formed might be exceptional, a series of thirty questions was prepared to be answered not only by the subjects of these experiments, but also by about three hundred boys and young men engaged in academic and collegiate study in various other cities of the Union. These questions refer to the signs of attention, the best time for memory work, the relative difficulty experienced in memorizing different disciplines, the place of repetition in memory, the means of steadying attention, individual mnemonic devices, and the influence of heredity upon memory.

On a Method of Studying Cerebral Circulation (the Eye-Plethysmograph). An informal communication. By E. C. SANFORD, Clark University.

The method in question is an insignificant modification of

one described by Dr. F. W. Ellis in the *Boston Medical and Surgical Journal* of April 21, 1887, but little known as yet to psychologists. It is in essence a means of taking pulse and blood-pressure tracings from the ophthalmic artery. This artery, which supplies the eye ball and orbit, is anatomically in direct connection with the cerebral system and furnishes an index of its condition.

To secure these tracings a plaster cast is taken of the upper part of the face, extending a little above and below the eyes and a little around onto the temples. In making this cast the eyes are covered with watch glasses, so that when finished, a hollow remains before each eye. Holes are afterward bored



The curves read from left to right. The upper one gives the time in seconds. The next gives the eye pulse; the third the respiration—upward movements showing inspiration, downward expiration; the fourth, the finger pulse—taken from the fingers of the right hand with a small air plethysmograph. The short vertical lines at the left give the relative positions of the writing points during the tracing of the curves. The irregular marks breaking into the ninth pulsation in the second and fourth curves show the momentary dislocation of the apparatus caused by the muscular 'start' of the subject, on hearing the sound.

The cut is a photographic reproduction, in the size of the original tracings, of a five-fold enlargement of them shown at the meeting of the Association. The enlargement was very carefully drawn for measurements of the curves to 0.1 mm., except in the case of the time line where a curve showing equal seconds was substituted for the actual one which contained 4.29+ vibrations to the second. The greatest irregularity in the motion of the kymograph in any second of the period shown was under one part in forty-seven.

into these hollows and short glass tubes inserted and sealed in place. When the cast is to be used the face is anointed with thick vaseline, to help in making the cast air-tight, and it is further held in place by a bandage about the head. Rubber tubes are slipped upon the glass tubes and their free ends lead away by a Y to a single Marey tambour. Every increase of blood in the orbit now causes a compression of the air in the cavities before the eyes, and an elevation of the stylus of the tambour which can be recorded in the usual way with a kymograph and smoked paper.

During the spring of 1896 this method was used by Mr. G. E. Dawson and the writer, in a study of the relation of certain psychical states to circulation and respiration. The accompanying cut shows a favorable, but by no means unique example of the tracings obtained. In it may be seen the rise of the cerebral blood pressure (second curve from the top) and corresponding fall of pressure in the fingers (fourth curve) caused by startling the subject with a sharp and unexpected noise. A full report of the study is delayed by the great time required for a careful study of the curves obtained.

Color-Blindness and William Pole: A Study in Logic. By MRS. CHRISTINE LADD FRANKLIN. Read by title.

Philosophy in the American Colleges. By A. C. ARMSTRONG, JR., Wesleyan University.

This paper was based on data obtained from a number of representative colleges and universities with reference to the development of philosophical instruction in recent years. It appeared that this development has been a marked one, although the movement has had a comparatively late origin. The causes of the increase in philosophical faculties and courses were discovered, first, in the general educational advance and the deepening of the national thought; second, in conditions special to the department. The growth of the special science has occasioned a demand for the speculative correlation and interpretation of their results. The progress of psychology, at first as an empirical and experimental science, then in relation to systematic discussions and even metaphysical problems, has given

it a central importance in the philosophical curriculum. Logic, on the contrary, has lost ground in comparison with its position a generation ago, and ethics, while better maintaining its place, has become more systematic and less practical, except in that political and social ethics have been added to the ethics of the individual. Apart from psychology, the greatest advance has been made in general philosophy. Little has been accomplished in the way of detailed historical research, but the history of philosophy, as now taught, gives the student some acquaintance with the history of intellectual progress as well as with the classical philosophical systems. Constructive thought and instruction have been begun, though the propounders of complete systems are few. Pedagogy is one of the youngest branches of the department, but not the least valuable.

With the extent and the content of philosophical teaching, methods have also changed. Elective courses have, for the most part, taken the place of prescribed work. In psychology emphasis is placed on experimental inquiry in the laboratory. Historical, systematic, ethical and religious philosophy take on a more literary form, with constantly increasing use of methods found fruitful in other departments.

In fine, philosophy has reëstablished its position in the American universities and may be expected more and more to influence our thought and civilization. [Printed in the *Educational Review* for January, 1897.]

Tests of Current Theory Touching Mind and Body. By
DICKINSON S. MILLER, Bryn Mawr College.

The probabilities and presumptions by which alone in this problem theory can be guided may be strictly tested and gauged:

I. As regards those cited on behalf of the theory of interaction: the evolutionary argument from 'the distribution of pleasures and pains' is invalid, but the evolutionary argument from the concomitant variations of mind and cerebral complexity and the original argument from the testimony of conscious experience do, indeed, yield positive presumption of a causal tie. Yet the presumption of unbroken physical order is also well-founded. Were the two incompatible the latter must give way.

They are, however, consistent. The facts in time or space are presumably as the parallelist states them; the material order is unfringed; nevertheless one psycho-physical event is the indivisible cause of the next psycho-physical event (the psychical and physical sides being by hypothesis inseparable), and hence the psychosis is part-cause of the total ensuing event, part-cause consequently of the ensuing neurosis. Thus the strict sense of our terms obliges us to admit an *unintrusive causation* on the part of consciousness and so to reconcile presumptions apparently at odds.

II. As regards universal parallelism or panpsychism; the arguments for it rest all upon the need of ascribing 'continuity' or 'uniformity' to the world. There are, however, four reasons for denying that panpsychism would yield such continuity. And even if it did, there prove on analysis to be no such signs of continuity in the world either of naïve imagination, of mechanical atomism, or of strict metaphysics, as to warrant the acceptance of a theory not otherwise evidenced.

The Relation of Mind and Body. By C. A. STRONG, Columbia University.

The object of this paper was to show that parallelism is not necessarily inconsistent with the efficacy of consciousness.

The interactionists usually fail to tell us what they mean by the matter on which they say that the mind acts. But matter means either a content of consciousness or an independent reality symbolized by that content, either object or eject. Furthermore, many idealists deny the existence of ejects; whence the following dilemma.

If ejects be denied, matter no longer remains in any palpable form for the mind to act upon; since it will hardly be maintained that our volitions act directly on the content of our perceptions. If ejects be accepted, there is no reason why our volitions should not act upon them and so deserve, in a sense consistent with Hume's doctrine of causality, the predicate of efficacy.

But such efficacy admits of, if it does not require, a parallelistic interpretation. For consciousness may itself be the eject

which appears to an onlooker as the brain process; and in that case the phenomenal interaction between the brain process and other physical events would run parallel to and be the symbol of a real interaction between consciousness and other ejects.

Is the 'Transcendental Ego' an Unmeaning Conception? By J. E. CREIGHTON, Cornell University.

This paper is an attempt to indicate some of the permanent elements of truth in the doctrine of the Transcendental Ego, taking into account, however, only the theoretical consciousness. There are two ways of looking at the knowing consciousness. First, that of psychology, which takes consciousness simply as a string of states or processes. These, as they first appear, are found to be complex and capable of resolution into elementary sensations. Now, the psychologist undertakes to describe the quality, intensity, extent and duration of the sensations, and to determine the various ways in which they combine into complexes. This investigation, being concerned only with the conscious processes and their modes of existence, finds no place for an Ego of any sort. But it is to be noticed, that so far as mental states are observed, as it were, from the standpoint of an external observer, the investigation deals with their *real*, not with their *ideal* side. And no description, however exact and accurate, of the various attributes of sensations, or of the ways in which they are fused and associated, can stand as an account of experience. To understand the fact of knowledge, then, a new standpoint—that of Logic or Epistemology—is necessary. Consciousness must here be conceived as taking the form of judgment. For knowledge comes only as the result of a process of interpretation and evaluation, and in this consists the essence of judgment. Knowledge, then, is the product of judging thought. Now the Transcendental Ego must be found in the judging thought or not at all. Both Kant and Fichte protest against making the Ego a thing or substance beyond consciousness. The real question is whether thought as we actually find it possesses the predicates—unity, identity, permanence, etc.—which the transcendentalists apply to their Ego. After showing in what sense we can speak of a conscious or ideal activity as

permanent and self-identical, the paper concludes with a statement of the positive grounds which compel us to apply these predicates to what we may call Thought or the Ego. Our experience forms one single system; the world of knowledge which is the product of the activity of intelligence is a whole, or at least *is required to be a whole*, and not a thing of shreds and patches. Now Kant argued from the unity of the Ego to the necessary unity of the Ego's experience. We may reverse the argument, and from the unity of experience infer that the thought which has constructed this experience is itself a single and self-identical principle.

The Relation of Pessimism to Ultimate Philosophy. By F. C. S. SCHILLER, Cornell University.

To show that the question of pessimism is an ultimate one for philosophy. Pessimism is not merely a possible outcome of the hedonistic calculus, but the denial that life is worth living may follow from the despair of *any* ideal of Value, *e. g.*, of Goodness, Knowledge and Beauty as well as of Happiness. It forms an attitude towards judgments of Value and the ultimate ideal of Value resulting from them, precisely analogous to the attitude of Scepticism toward judgments of Fact and the ideal of Truth. In each case there seems to be three possible attitudes: affirmative (gnostic—optimist), negative (sceptical—pessimist), and agnostic (or 'critical'). But the third may be reduced to the second. Further, the question of ultimate Fact is finally subordinate to that of ultimate Value, so that the question of Optimism or Pessimism becomes the final alternative for Philosophy. Pessimism, however, remains secondary. Practically, the recognition of this view would strengthen Philosophy.

The Method and Standpoint of Ethics. By JAMES SETH, Cornell University.

The present tendency to regard Ethics as a science rather than as a part of Philosophy or Metaphysics is a reaction from metaphysical Ethics of Kant, and a return to the sounder view of Aristotle and of the earlier British school. We must, however, distinguish two types or groups of science, the Norma-

tive and the Natural. The normative sciences deal with our judgments of worth, the natural sciences with our judgments of fact. To the former class belong Logic, *Æsthetics* and Ethics. Our several judgments about the value of thoughts, of feelings, and of actions are reducible to a common denominator of Truth, of Beauty and of Goodness; the discovery of this unifying principle and the construction of the system of our intellectual *æsthetic* and ethical judgments in their organic relation to it, is the business of Logic, of *Æsthetics* and of Ethics respectively. These sciences must be distinguished, no less than the natural sciences, from Metaphysics, whose province it is to deal with the question of the ultimate validity of our judgments, whether they are judgments of fact or judgments of worth. Both the natural and the normative sciences have to be criticised and correlated by Metaphysics, whose question of questions is that of the comparative validity of the Ought-judgments and the Is-judgments as expressions of ultimate Reality. The distinction here insisted upon between the normative and the natural sciences is not a difference in method, but only in subject-matter. The function of Ethics, for example, is like that of Physics, merely to organize the judgments of 'Common Sense' or ordinary thought. There is a 'Common Sense' of value, as there is a 'Common Sense' of fact; and there is a science of value, as there is a science of fact. It is not possible for Ethics to transcend the sphere of Common Sense, and to discover, beyond that sphere, a Norm or standard by which we can establish or invalidate the judgments of Common Sense. Like all sciences, Ethics is a criticism of Common Sense; but it is an immanent criticism, a self-criticism.

A Generalization of Immedite Inferences. By JOHN GRIER HIBBEN, Princeton University.

When we have given, All x is y , it is possible to infer immediately:

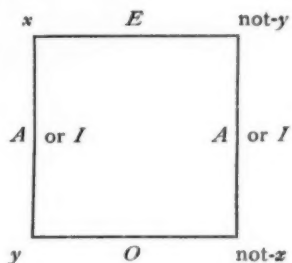
- (1) The Converse, Some y is x .
- (2) The Obverse, No x is not- y .
- (3) The Converted Obverse, No not- y is x .
- (4) The Contrapositive, All not- y is not- x .

(5) The Obverted Converse, Some y is not not- x

(6) The Inverse, Some not- x is not y .

(7) The Obverted Inverse, Some not- x is not- y .

The above may be tabulated in the following square of Immediate Inferences:



The letters A , E , I , or O indicate that the two terms between which any of them is situated may be joined in a proposition of the form represented by that letter; and in every case such a proposition will be a legitimate inference from the original proposition, All x is y .

Thus between the two upper terms x and not- y , two propositions of the form E may be inferred:

No x is not- y . E .

No not- y is x . E .

Between the two lower terms, two O propositions are possible:

Some y is not- x . O .

Some not- x is not y . O .

Reading down the two vertical lines, two A propositions:

All x is y . A .

All not- y is not- x . A .

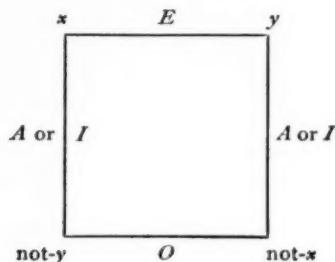
Reading from the lower to the upper two I propositions:

Some y is x . I .

Some not- x is not- y . I .

Again when E is the original proposition, no x is y , all the possible inferences may be comprehended in a square which dif-

fers from the one above only by interchanging the positions of y and not- y . This is in accord with the fact that an A proposition becomes an E proposition by obversion, in which process it is observed that not- y displaces y . Given no x is y , we have :



Forming propositions as before we have all the inferences from an E proposition :

No x is y . E (the original proposition).

No y is x . E

Some not- y is not not- x . O .

Some not- x is not not- y . O .

All x is not- y . A .

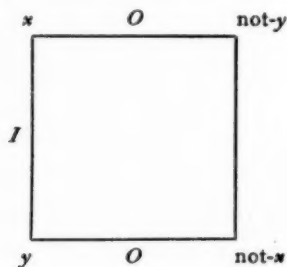
All y is not- x . A .

Some not- y is x . I .

Some not- x is y . I .

When I is given, some x is y . We have the A and E inferences of the A square becoming I and O respectively; also the horizontal lines are to be read from left to right only; and no inference is possible between not- x and not- y .

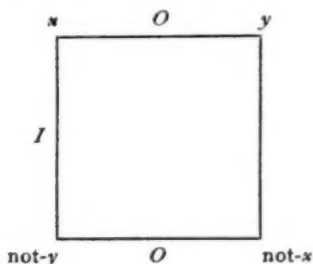
We have therefore the following :



The possible propositions are :

- Some x is y . *I*.
 Some y is x . *I*.
 Some x is not not- y . *O*.
 Some y is not not- x . *O*.

Similarly when *O* is given, some x is not y , the square is the same as the *I* square with the interchange of y and not- y ,



The possible propositions are :

- Some x is not y . *O*.
 Some not- y is not not- x . *O*.
 Some x is not- y . *I*.
 Some not- y is x . *I*.

[NOTE.—When *A* or *E* is inferred, the weakened form *I* or *O* is always possible.]

The Negative in Psychology and Logic. By A. T. ORMOND.
 Princeton University.

The paper is summed up in the following propositions :

I. That the theory of negation is involved in the general theory of judgment, which must be considered as a preliminary.

II. That all judgment arises out of volitional grounds and maintains the volitional form in its central pulse of assertion.

III. That the differentiating conditions of judgment are : (*a*) the rise of the world of representation as objective, (*b*) the presence of some interest in this objective world, (*c*) the real subject of the judgment which is some body of experience or knowledge, constituting the genus or universal within which the judgment function acts.

IV. That all judgment with the possible exception of the simple existential is disjunctive, in the sense that it is concerned with alternatives in a larger universe in which these are included.

V. That negation arises along with affirmation as a form of the self-assertion of the real subject. The assertion of this subject motives to the affirmation and negation.

VI. That denial arises on the presentation of the incompatible to this subject, and is simply the act of removal or suppression by means of which the subject maintains itself.

VII. That denial does not necessarily presuppose a previous affirmation, actual or supposed, but may arise directly as a reaction against the incompatible, just as we may assert directly the inequality of two lines.

VIII. That the *function* of denial must be distinguished from its *implications*. In its function denial is always removal and can never pass into affirmation. By implication, however, affirmation is involved in various degrees in denial.

IX. The negative affects the copula of a judgment rather than its predicate. The theory that the negative is not copular virtually abolishes negation by translating all judgment into the affirmative form.

X. But thinking cannot get on without denial. In the world of alternatives the incompatible arises. In presence of the incompatible, affirmation is powerless. The pulse of denial is as essential to thought and knowledge as is that of affirmation.

XI. But while the functions of affirmation and denial forever remain distinct, they tend, as the body of knowledge increases in scope and definiteness, to become more and more closely connected in their implications.

This paper will appear in full in THE PSYCHOLOGICAL REVIEW.

Address of the President. Subject: *The 'Knower' in Psychology.*

By GEORGE S. FULLERTON, University of Pennsylvania.

An examination of the treatment of the Self in its function of 'knower' by philosophers and psychologists, and a discussion of the meaning of 'knowledge' in psychology. It

was an attempt to apply to this problem the scientific psychological method outlined as the true psychological method in two earlier papers read by the author at recent meetings of the Association. It was shown, in a brief historical sketch of the development of the idea of the Self in the history of philosophy, how the abstract and inconsistent notions which usually pass current have come into being. The author then turned to modern psychology, and first criticised the notion of the Self as a self-constitutive activity, which comes to the surface in the writings of the Neo-Kantian school, finding the position of its advocates inconsistent and untenable. He next took up the view of the Self that regards it as a noumenon, or a something to be distinguished from the phenomena of consciousness, and in some sense underlying them, taking as the chief subject of his criticism the recent works of Professor Ladd, which do not, it is true, hold to a noumenal self in a bald and uncompromising form, but which, in his opinion, combine this notion with that held by the Neo-Kantians, and with a third, which he discussed later. He held that this position necessarily leads to difficulties which prove its untenability.

Following this, he referred to a class of thinkers which he described as belonging to the Humian school, using that term in a broad sense to indicate those who repudiate noumena, and accept the phenomena of consciousness and their inter-relations as furnishing the whole material with which the psychologist has to deal. He referred particularly to the later writings of Wundt, and those of Ziehen, Külpe and Titchener. With the general position taken by these authors he expressed himself as in substantial harmony, although he did not regard them as having seen the full significance of their own teachings on the question of the nature of knowledge either for psychology or epistemology.

The latter part of his address, embodying his own positive conclusions, maintained that the attempts to explain knowledge by the intervention of a 'Knower' of either the noumenal or the Neo-Kantian sort are based upon a misunderstanding, and regarded the notion of the abstract and inconsistent 'Knower' still current among philosophers and psychologists as a survival

from, and development of, the crude notion of the bodily self which precedes the beginning of reflection, and the duplicate of this seen in the animism of savage races.

[The address has been published in full in *THE PSYCHOLOGICAL REVIEW* for January, 1897.]

PRELIMINARY REPORT OF THE COMMITTEE ON PHYSICAL AND MENTAL TESTS.

The Committee on Physical and Mental Tests appointed at the last annual meeting of this Association submits the following report:

The committee has drawn up a series of physical and mental tests which is regarded as especially appropriate for college students tested in a psychological laboratory. The same series would also be suitable for the general public and, with some omissions and slight modifications, for school children. The committee has had in view a series of tests requiring not more than one hour for the record of one subject. In selecting the tests and methods the committee regarded as most important those which seemed likely to reveal individual differences and development, but also took into account ease and quickness in making the tests and in interpreting and collating the results.

Each member of the committee selected a tentative series of tests. The report includes these selections, together with brief descriptions of methods. After each test and method are placed the initials of the members of the committee recommending it.*

* We refer especially to two publications for descriptions of some of the tests: *Official Catalogue of Exhibits*, Department M., World's Columbian Exposition, *Section of Psychology*, Joseph Jastrow in charge, 1893; and *Physical and Mental Measurements of the Students of Columbia University*, J. McKeen Cattell and Livingston Farrand, *THE PSYCHOLOGICAL REVIEW*, Nov., 1896. The following papers on the subject may also be mentioned: "Mental Tests and Measurements," J. McK. Cattell, with an appendix by Francis Galton, *Mind*, 1890; "Zur Individual Psychologie," Hugo Münsterberg, *Centralblatt f. Nervenheilkunde und Psychiatrie*, 1891; "Researches on the Mental and Physical Development of School Children," J. A. Gilbert, *Studies from the Yale Laboratory*, 1895; reported also by E. W. Scripture, *Zeitschrift f. Psychologie*, etc., X., 1896, and *THE PSYCHOLOGICAL REVIEW*, III., 1896; *Der psychologische Versuch in der Psychiatrie*, Emil Kraepelin, *Psychologische Arbeiten*, 1895; *La psychologie individuelle*, A. Binet et V. Henri, *L'Année psychologique*, 1896.

Preliminary Data: B. C. J. S. W.

Date of birth; birthplace; birthplace of father; birthplace of mother; occupation (including class in college, or, if not a student, the last educational institution attended); occupation of father; any measurements previously made. B. C. J. S. W.

Color of eyes; color of hair; right or left-handed. B. C. J. S.

Mother's maiden name; number of brothers; sisters; order of birth; age of parents at birth; birthplace and occupation of grandparents. W.

Two schedules of observations and records to be filled in, one by the recorder and one by the subject, as in the Columbia tests, with such modifications as experience shall make desirable. C.

A blank to be filled in by the recorder, noting asymmetry of head or body, color of eyes and hair, complexion, degenerative or other stigmata of head, eyes, ears, mouth, teeth, hands or feet, posture, gait, manner, coordination and speech, indications of intellectual, emotional and moral characteristics. W.

Physical Measurements: B. C. J. S. W.

Height, weight and size of head. B. C. J. S. W.

Breathing capacity. C. J. S. W.

Height sitting. C. W.

The measurements should be made in the metric system. The weight should be taken in ordinary indoor clothing. The height should have the height of the heel subtracted. At least the length and breadth of the head should be measured. B. C. S.

Keeness of Vision: B. C. J. S. W.

The maximum distance at which diamond ($4\frac{1}{2}$ point) numerals can be read with each eye singly. B. C. J. S.

The illumination should be in the neighborhood of 100 candle-meters; about eight out of ten numerals should be read correctly at the rate of about 2 per second. The minimum distance should also be determined, if possible. B. C.

In addition or as a substitute, drawing a series of forms as recommended. J.

Use Snellen Test-types. B. S.

Some other substitute for these tests, to be suggested after satisfactory trial. W.

Color Vision: B. C. J. S. W.

Select as quickly as possible four greens from a series of wools; measure the time; if long, make further tests. C.

Combine with test of rate of perception by requiring subject to name, as rapidly as possible, a series of colors, either wools or papers. B. W.

Use the chart exhibited at the World's Fair. J.

Keeness of Hearing: B. C. J. S. W.

The distance at which a continuous sound can be heard with each ear singly. B. C. W.

Use some artificial external meatus if the test is to show small differences in sensibility. W.

The sound should be from a watch reduced to a standard. An arrangement should be used by which it can be periodically cut off without the knowledge of the subject. C. S.

Use for this a stop-watch. B. J. S.

I endorse the stop-watch; it can be manipulated so that the time is recorded, showing how long it took the subject to decide that the watch has stopped. J.

Perception of Pitch: B. C. J. S.

Adjust one monochord or pipe to another, the tones not to be sounded simultaneously. C. J.

Select a match from a set of forks, making a fixed number of vibrations per second more or less than a standard, *e. g.*, standard 500 v. per second; other forks 497, 497.5, 498, 498.5, etc.; 500, 500.5, 501, etc. B. S.

I prefer the adjustment to the selection method. The test can be made with two Gilbert tone-testers. J.

Fineness of Touch: C. J. S. W.

The æsthesiometer is unsatisfactory; the discrimination of roughness of surfaces and touching a spot previously touched should be tried. C. J. W.

Sensitiveness to Pain: B. C. J. S. W.

The gradually increasing pressure that will just cause pain. The point or points in the body to be used to be agreed upon. B. C. J. W.

Perception of Weight or of Force of Movement: B. C. J. S. W.

Arrange a series of weights. B. J. W. With and without sight. B.

Make movements of equal force and determine the error. C.

The best method still to be developed. J.

Dynamometer Pressure of Right and Left Hands: B. C. J. S. W.

In place of or in addition to the ordinary dynamometer test make movements of the thumb and forefinger and continue as rapidly as possible for fifteen seconds. B. C.

Use mechanical counter for this and take reading at end of every minute. S.

Thumb and finger dynamometer. Record best and worst of five trials. W.

Rate of Movement: W.

Distance of 35 cm. One preliminary trial with right hand in extension, then two trials in succession of R. E., L. F., L. E., R. F. Collate shortest of two trials under each typical movement. W.

Fatigue: B. C. W.

Muscular exertion. B. W. As described above. C.

Intellectual exertion. B. W.

Will Power: W.

The ability of the subject to respond after fatigue has set in to a suggestion of the experimenter with an extra effort of will. W.

Voluntary Attention:

Test by simple mental operations under distraction. B.

Coincident variations in Psycho-physical process. W.

The modifiability of the knee jerk, or of a sustained bodily process, such as rate of breathing or pulsation of a volitional muscular or intellectual process, when the subject's attention is engaged by some mental content. W.

Measure at the same time concentration or distraction of attention. W.

Right and Left Movements: J. W.

The accuracy with which movements are made to the right and left. J. W.

Some such test as this for indication of right and left-handedness. W.

I do not insist on this test as one of great importance. J.

Rapidity of Movement: C. J. S. W.

Taps on a telegraph key. J. W.

Movements requiring force, as described above. C.

Make short marks as rapidly as possible for twenty or thirty seconds, *e. g.*, | | | | |. S.

Trilling with two fingers and with five. W.

Accuracy of Aim: B. J. S. W.

Throwing a marble at a target. J.

Or striking a point upon the table with a pencil point. W.

Touch an insulated spot, as proposed by Scripture. S. B. Also for steadiness of hand. B.

Reaction-time for Sound: B. C. J. S. W.

The reaction to be made with the right hand with a signal about two seconds before the stimulus. B. C. J. W.

Five reactions to be made without preliminary practice; after the reactions have been made, the observer to be asked whether the direction of the attention was motor or sensory. B. C.

It is not much use to ask for direction of attention with most subjects. W.

Sensory and motor reaction with instruction, after the above test. B.

Reaction-time with Choice: B. J. W.

Use card sorting. B. J. S.

Rate of Discrimination and Movement: B. C. J. S. W.

100 A's in 500 letters to be marked or as many as can be marked in one minute. B. C.

One out of a number of geometrical forms to be marked: determine the number marked in 90 seconds. J. W.

Or colors, or pictures of objects. W.

Quickness of Distinction and Movement: B. J. S.

Rate at which cards are sorted. B. J. S.

Combine with reaction with choice. B.

With the effects of practice, etc., as proposed by Bergstrom. S.

Perception of Size: C. J. S. W.

Draw a line equal to a model line 5 cm. in length, bisect it, erect a perpendicular of the same length and bisect the right-hand angle. C. J.

Perception of Time: B. C. J. S. W.

The accuracy with which a standard interval of time, say ten or twenty seconds, can be reproduced. C. W.

Thirty seconds or one minute. W.

Memory: B. C. J. S. W.

The accuracy with which eight numerals heard once can be reproduced and the accuracy with which a line drawn by the observer

at the beginning of the hour can be reproduced at the end of the hour.

C. W.

Line to be identified (not drawn). Ten numerals to be used. B.

Nine numerals. S.

A combined test of memory, association and finding time as described in the catalogue of the Columbian Exposition. J. W.

Accuracy of observation and recollection as proposed by Cattell and by Bolton. J. W.

Memory-type: B.

Variations in use of 10 numerals; method as follows:

1. Show numerals in chance order and have subject write them from memory after a small interval.

2. Speak numerals in chance order and have subject write them from memory after the interval.

3. Show and speak in chance order and have subject write them from memory after the interval.

4. Show and have the subject speak them and then write them from memory after the interval.

Compare the results for indications of memory type and kind of imagery preferred. Question the subject as to his mental material in each case. B.

Apperception Test of Ebbinghaus. B.

Imagery: B. C. J. S. W.

Questions proposed in the Columbia tests. C.

Methods should be worked out more fully. B. C. J. W.

Cf. Method under preceding head. B.

Make memory span tests, showing and speaking the digits at the same time, and ask the subject which sense (sight or hearing) he found himself using, and if either seemed to him a distraction. S.

The committee urges that such tests be made, so far as possible, in all psychological laboratories. It does not recommend that the same tests be made everywhere, but, on the contrary, advises that, at the present time, a variety of tests be tried, so that the best ones may be determined. Those who make tests which they regard as desirable are requested to send these with sufficient description to the committee.

The committee hopes that the tests proposed may be dis-

cussed fully at the present meeting of the Association, and asks that the present committee be continued for another year.

(Signed,)

J. MARK BALDWIN,
JOSEPH JASTROW,
E. C. SANFORD,
LIGHTNER WITMER,
J. McKEEN CATTELL, *Chairman.*

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or addresses as given in the above list.

UPRIGHT VISION.

BY PROF. JAMES H. HYSLOP.

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The present paper discussing the problem of upright vision directly has been suggested by what I have already said in a previous number of the *PSYCHOLOGICAL REVIEW* in regard to Dr. Stratton's experiments. I wish here to discuss the whole question on its own merits and without regard to anything that has occurred in that connection, and the first task must be to show just what the problem really is. In order to do this effectively it may be well to look a moment at its origin, after stating the form in which the question is usually put for an answer. It is: "Why are all things seen upright when the image on the retina is inverted?"

Before I get through I shall endeavor to show that this way of putting the question easily leads to an illusion in regard to the nature of the problem, but for the present, taking it as perfectly intelligible, we may further ask how such a question ever came to be put at all? No one ever thought of asking it until the discovery was made that the eye is really a *camera obscura* which inverts its images. The problem created by the emanating corpuscles of Democritus and by the etherial vibrations between the object and the eye did not suggest it, and probably could not, as the formation and inversion of the image was not known until the property of lenses was known. Nothing anomalous in the phenomena of vision was suspected before the fact of refraction was recognized. But as soon as it was discovered that the image on the retina was inverted, the apparent anomaly at once suggested the question why the object was seen upright when the image is inverted, and various theories have been invented to explain the phenomenon. Among them we have the ocular movement theory, the projection theory, the re-inversion

theory, and the theory of correction by experience in connection with touch. The last theory holds that in respect of visual functions, the inversion of the retinal image is not necessary to upright vision, but that the conception of uprightness is the product of experience and that naturally we may or do perceive things upside down.

This view of the case, and the question creating the problem or supposing that the phenomenon is anomalous, are founded upon two illusions. The first of these illusions is that, to be intelligible to our minds, the process ought not to involve an inversion of the retinal image. Until it was discovered that the eye was a camera it was natural to conceive the process after the analogy of touch, this conception being modified by the idea of vibrations. There were metaphysical and idealistic puzzles enough in the phenomenon, and perhaps also epistemological problems, but more for psychology or perception as a fact. In touch we were accustomed to a perfect correspondence between the impression upon the sensorium and the object producing it. There was apparent no disparity or inversion of relations. The space relations of impression and object were taken or known to be symmetrical and nothing seemed to be anomalous about them. But as soon as it was found, as a matter of fact, that the retinal image is inverted—that is, its position reversed from that of the real object—the question arose how we could see the object upright.

Now it is to be noted that the question was not, how this retinal image becomes inverted, because we have no immediate knowledge of the fact, and so it could not be a problem. Its existence was simply inferred as a necessary result of what is known of the property of lenses, though it was easily confirmed by experiment after the fact was inferred as a consequence of the nature of the eye. Hence the reason for the inversion of the image was explained before its existence could be demonstrated empirically, or at least it could be explained. Hence the anomaly did not consist in the fact of inversion, but in the relation of this fact to the opposite relation of the object, and the question arose, How does perception take place under these conditions? Or why do we see the object in an upright position when the retinal image is inverted?

*One thousand leagues under
the sea!*

Ditto.

But what precisely does the question mean? Does it ask for the explanation of an anomaly? After all, is not the question essentially absurd? It certainly does not appear so to those who ask it. But the reason is an unconscious assumption which creates the whole problem to be solved, but which may have no ground upon which to rest. It is the assumption that in order not to be an anomalous phenomenon the object and the image ought to correspond. The old Democritean view of perception involved precisely this conception of the case: the εἰδωλα imprinted themselves on the eye, we should say retina. The undulatory theory of light hardly altered the conception, except for metaphysics. There was here the idea of correspondence between image and object, whether rightly or wrongly assumed. Of course, as soon as this assumed correspondence was disproved, there would arise the conception of something anomalous, and the question mentioned would arise. As long as the phenomenon of vision did not appear thus exceptional, there would not be anything to suggest that it was a non-natural fact. Upright vision would be taken as a matter of course. But the moment that it appeared as an exception to what it was supposed it ought to be, instead of supposing that this was just as natural as any other process, the assumption was made that we ought to see things the inverse of what we do as a fact, and that experience corrects the illusion. Instead of supposing that the old assumption or conception of the process was an illusion, men were disposed to accept it as the standard by which to judge the anomalous character of the facts, and rushed off to experience to correct or change what was supposed to be the natural perception of the infant!

I do not say that all persons conceived the question exactly in this way; for there were some who still supposed the process to be natural enough, but regarded it nevertheless either as anomalous or as presenting special features which required explanation. Where it was felt to be anomalous, even if natural, it was judged somewhat after the analogy of touch. In this sense the correspondence between the impression and the object was such as to bring into bold relief the opposition in vision; and the natural tendency was either to go to experience for the

adjustment of the two sets of phenomena to the same law, or to some anomalous function in sight to compensate for the deviation of the law of touch. This was making touch the standard of what is natural, just as Berkeley did in the matter of space perception. But what right have we to judge vision by *any* such assumption? Might not upright vision be just as natural with the inversion of retinal images as the symmetrical relation in touch. Why should we judge the law of one sense by that of another? Does not the action of each sense suffice for itself, and is it not an assumption requiring justification that the process of vision may be rendered intelligible by tactual analogies? There is a deep-rooted, and perhaps legitimate, impression that there is some sort of unity between the senses, and this we may be able to establish in the conclusion. But no such unity can be assumed as would either identify their percepts or necessitate the same law of action between them, and as long as this is the fact there will be no intrinsic reason in the fact of inverted images to justify our assumption, so frequently made, that it is anomalous, and contrary to our ordinary notion of perception as drawn from the impressions of touch. That this comparison should be made is an illusion, perhaps pardonable enough, but still an illusion.

This illusion is reinforced, perhaps in some cases created, by a very interesting ambiguity. This is in the conception of 'uprightness.' The idea of 'uprightness' denotes a relation to the earth, which is assumed to represent the natural and uniform position of bodies. I might have said a relation to gravitation, which undoubtedly expresses the case for most persons, who come to know, that uprightness is merely relative to a point at the earth's center, and determined by gravity, and not an absolute position in space. Now the most invariable of all the experiences by which we estimate the direction of gravity is our own sense of weight. We can determine it by the visual perception of falling bodies, but movements in the visual field, until we learn the ultimate influence of gravity on the bodies thus moving, would hardly suggest gravity so soon or so forcibly as the absolutely constant sense of weight and the limitations upon free personal movement in space. Then the final discovery

from the general result of all experiences, that gravitation defines a line of direction or position for all bodies in relation to the earth, creates the idea that 'uprightness' is this line and it becomes convertible with the tactual and muscular sensations which are the constant and surest determinants of it, though vision gives a line of direction which coincides with it. But we conceive it as related to gravitation rather than as any mere fact of the visual field alone. It is true, however, that the idea of uprightness can have the same meaning for vision as for touch, in the sense that it means merely a reference to the direction of gravitation. But this is not because the sensations or experiences determining it are in any respect like each other. It is because one of them may always be taken as the associate of the other, or as an index of its possibility. They become associated by their synthetic unity in perception, and when any question is raised as to the validity of one of them it is natural to refer to its associate as an index of what the one in question means. In this way, when the question how we perceive uprightness under the conditions mentioned is raised, in view of the sceptical implications of the question itself, and the desire to obtain an explanation without simply restating the fact itself in a mysterious form, it was natural to resort to the tactual process for the datum with which the visual sensation is associated and which represents the uprightness. But the fact is that the visual conception of uprightness is just as definite a content of visual experience as the tactual is of touch, and it would never have any connection with the latter but for the uniformities of certain experiences. This aside, however, the fact that vision can determine independently of touch a relation to gravitation, as a notion of uprightness, is sufficient to show that the datum is not necessarily tactual, so that, however valuable it may be to use touch for explaining the synthetic conception of uprightness, it still remains to explain the visual process within the limits of that sense.

But this reference of the term uprightness to the direction of gravity, or the direction which gravity determines, and to the same relation expressed in tactual and muscular experiences of a certain order, leads to an entire misunderstanding of the real question by making us think of uprightness as felt, when the

visual problem uses the term, not necessarily to express the direction of gravity in harmony with touch, but the inverted position of that relation in the image, whether it represents a line in reference to gravity or not. It happens in normal experience that the objects we see in vertically linear extension are arranged with reference to the direction of gravity and we call them 'upright.' That is, visual uprightness and the uprightness of gravity coincide, while that of touch coincides with the same objective reality, and when the retinal image appears inverted we ask how the object can be seen in the inverse position under these conditions, but instead of saying 'inverse' we say 'upright' and create the liability of confusion either with tactual and muscular uprightness or with the adjustment of tactual and muscular experience to the visual. The way to avoid this confusion is to examine the phenomena themselves upon which the problem is based and to remark that it can be stated without using the word 'upright' at all. Now when we note the relation between the object and the retinal image we do not require to ask how we see things upright, but how do we see things in a certain relation when the retinal image represents the inverse relation. The term 'upright' does not appear here, nor is there any reference to the real direction which it represents in relation to either gravity or the other senses. In the visual problem, therefore, the term ought either not to be used at all or it must be understood to mean nothing but the inverse of the relation of the image on the retina. In this way we see that all the associated conceptions of other senses are excluded, and there remains nothing but the real or apparent anomaly of opposition between the position of real objects and that of retinal images.

I have dwelt at length on the illusions and the true conception of the problem, because it is necessary to clear away, once for all, all those experiments and theories of the process which are determined by a reference to tactual and muscular adjustments to visual objects. They are designed to prove that the inversion of the image is not normally necessary to the perception of things as we see them, but that when reversed by artificial means we easily learn to adapt ourselves to these new conditions.

The adaptation is true enough as a fact, but it is not relevant to the problem. Nor in speaking, as I do, of what such attempts at experiment and theory imply in favor of empiricism, do I mean to imply my own denial of the influence of experience in correcting what might be the original appearance of things to sight. I mean only to state the case so that we can see that, however much experience may have to do with the adjustment of touch to the visual field, we have not in this any fact from which we can justly infer that experience corrects the original perception of vision and enables us to *see* things the inverse of what they once appeared, as it would be necessary to maintain if we affirmed the proposition that the inversion of the image is not normally necessary to upright vision. Within the limits of sight a man may hold what opinion he pleases on the point. I am not at present denouncing empiricism within such limits, but only the inference from the experiential nature of adjustment to sight to the experiential nature of the process in sight. Whether the visual process is native or empirical will have to be determined by phenomena wholly within the range of that sense, and it is an entire misconception of the problem to talk about any tactual or muscular adjustments to visual impressions. Ocular movements might appear relevant, but no others are relevant that involve a visual judgment of co-existence in space.

Now, having shown that tactual experiences are not relevant, but give rise to an illusion in the problem, we may examine the assumption of empiricism wholly within the limits of visual phenomena. If we assume that experience corrects the original perception of vision by supposing that the inversion of the retinal image is not necessary to 'upright' vision, we imply that originally there was complete correspondence between image and object, as between tactual impression and object. This is the assumption of a created function wholly supplanted by experience. But what facts justify such an assumption? There is absolutely no fact to justify it except the *a priori* idea of the analogy between touch and sight, or those stories which we occasionally hear about the inversion of objects in case of disease, which require to be much better verified and analyzed be-

fore they are entitled to use in this connection.¹ We should never suspect the influence of experience in the case were it not for the unwarranted and unjustifiable assumption following in the wake of the theories of Democritus and the undulatory nature of light. Here we took for granted the identity of the relation between impression and object in the senses of touch and sight, and allow an apparent anomaly to convince us of the influence of experience, when we might just as well have abandoned the assumption of the nativity of the correspondence between them. The fact is that it was experience and *a priori* reasoning that led us to suppose that the phenomenon was anomalous at all, when we ought to have seen that it represented the natural condition of things, and that any correspondence in symmetry between retinal images and objects, if possible at all, would have to be either the product of experience or the result of abnormality.

¹ I have often heard second and third-rate stories about persons who, under certain abnormal conditions, actually saw things upside down. But I have to say that all attempts that I have made to secure a perfectly authentic case have utterly failed. Persons who have told me of them could not vouch for them, and I always find them eluding investigation much as do ghost stories. I have never found any reference to them in books, and Dr. Peterson, of the College of Physicians and Surgeons, in connection with Columbia University, allows me to quote him as saying that, in all his study of nervous diseases and reading, he has not met with a single case of it, and would not believe the narrative if he did meet one. Two years ago I thought I had secured a case on the testimony of a physician. On careful interrogation it turned out that his observation had been limited to the fact that a young boy in Brooklyn had been known always to write and read upside down and experienced great difficulty in correcting his habit. This had occurred twelve years before, and the boy could not be traced. But it is evident that he did not require to be traced, because the phenomenon does not present the slightest evidence of inverted vision. I would ascertain nothing about the boy's habits previous to his entrance to the public schools. Children taught the alphabet and to spell upside down will read by holding the book in an inverted position. But this does not prove inverted vision, and I suspect most reported cases are of this kind. Moreover, any reported instances of a pathological kind have to be viewed with suspicion, for obvious reasons. One instance, however, and this an apparent exception to all my experience, came within my knowledge a short time ago. A lady told me that she had often seen things upside down. On interrogation it appeared that she could not give any clear account of her experience. She remarked that the phenomenon was always connected with very severe headaches, which were often almost unendurable and blinding. She said also that she could immediately correct the impression by reaching out to the object with her hand, and that the whole field did not seem inverted, but only

I have dwelt at length upon the analysis of the problem partly for the purpose of limiting it to its proper field, and partly for the purpose of clearing away its imaginary difficulties and defining exactly what an explanation of the phenomenon must do. It is usual with the empiricist to be content with a reference to some fact or process which involves the influence of tactual or muscular experience. He implies, if he does not assert, that we either correct a primary illusion by this process or we never obtain any properly visual *quale* which has a right to be called uprightness. In this way he conceives the whole problem as one regarding the *genesis* of the idea of uprightness in a sense that does not naturally give it or is supposed not to give it. Whatever he thinks about the naturalness of the percept in the tactual and muscular sense, he can only conceive sight as giving signs which may be used as data for inference to the existence of certain tactual and muscular relations. But he construes the whole question as if it were

certain objects in it, though I could not get any satisfactory account of the experience in its details. On the whole, however, I found nothing in the case that might not be explained by an illusion of judgment in connection with the mental confusion incident to severe headache. Now it is to be remarked that if the whole field appeared inverted in the case the fact could not be discovered except in comparison either with a memory image or with disturbed coenæsthesia and memory images together. But this would not require any inversion of the apparent object to vision, but would only show an inversion of the feelings that serve as a criterion of the relation between memory images and those feelings. Moreover, touching the object would not correct the impression of sight if it represented an organic disturbance or inversion of retinal impressions. It would only correct her judgment. This is seen in the ordinary experience with the microscope, where we easily correct our judgment of locality for touch, without altering the *seen* relations of space in the object. It is worth remarking also that in microscopical experiments we should never suspect the inversion of objects, except for the fact of memory images with which they are compared. Moreover, if, in the case under consideration, only the object looked at appeared inverted, this fact and its correction immediately by touch would prove that the case was an error of judgment and not organic inversion of images. Otherwise we should have to suppose organic disturbance for the one object while the remainder of the field was normal, and it would require a great deal of evidence to support such a fact. I have, however, treated this case more seriously than it deserves, but only because it is the single one in my experience which could claim a moment's scientific attention. There is nothing in it which cannot be explained by supposing an illusion of judgment instead of an inversion of visual reference.

merely one between the theories of nativism and empiricism, when, as a matter of fact, this is not the proper way to conceive it. This may be an interesting question, but what we want to know first is the law of sensorial reference which either explains upright vision, that is, the inversion between image and object, or proves it to be anomalous. If we prove it to be anomalous we suggest a reason for resorting to foreign and empirical influences as secondary agencies in the matter. But whether we ever find it necessary to discuss nativism or empiricism in connection with it, it is certain that we should first ascertain, or exhaust every effort to ascertain, the conditions in the sense of vision that may explain the phenomenon. That we are under obligation to do this is shown by the fact that there is an absolute universality of agreement in visual experience regarding the position in which things are seen (barring possibly abnormal cases above referred to). That is, no one ever remembers a time when his visual judgments required correction, except in reference to memory images, and no illusions occur in normal experience, as they should do, if empirical theories were true. The uniformity of experience in the matter, negatively confirmed by apparent exceptions which will not bear investigation, only indicates or proves that we must seek some conditions within the sense of vision to explain this regularity and exemption from illusion.

All such theories as re-inversion of the image or mysterious central agencies may be thrown out of court at once as simply a restatement of the problem at a point where it cannot either be investigated or subjected either to proof or disproof. The two most prominent explanations have been the ocular movement and the projection theories, which represented respectively the empirical and the nativistic points of view, and whose examination will show that they either begged the question or simply restated the question in more mysterious terms.

The ocular-movement theory supposes that we learn the direction of a point in the field of vision, or rather its position above or below, by the movements necessary to bring it upon the fovea. But this supposition will not stand a moment's examination. It has to assume either a perception of the point to which

the movement of the eyes has to be directed or a consciousness of the relation between this movement and the tactual percept of position and uprightness. To assume the first of these alternatives is to admit the existence in consciousness of the datum which has to be derived from the muscular sensations in ocular movements. That is to say, it admits a perception of the uprightness before the ocular movements can have any meaning for consciousness at all. The eye may not yet know that the positions perceived correspond to certain directions represented by gravity—that is, it may not have identified visual with tactual uprightness—but the relations are given in the visual percept or manifold which determine the meaning of the ocular movement and are not determined by it. The fact is, moreover, that in all ordinary processes of ocular movement we have no knowledge of such movements directly, but only of the objects across the field of vision. There is not even a muscular sensation to serve as a $\pi\omega\tilde{\upsilon} \sigma\tau\tilde{\omega}$ for judgment, except in extreme or strained positions or movements of the eyes, and it is specially noticeable in these conditions that the perception of direction or uprightness is not made any more evident by it. Hence the fact is that the direction of ocular movement is determined by the previous perception of relations which the theory assumes are determined by the movement, the sense of movement being known only in the changes across the visual field, and not in the muscular sensations. To assume the second alternative, which involves a knowledge of the relations between the movement and the tactual percept of uprightness, is to make matters worse still. For nothing is clearer than the circumstance that we do not learn the fact of ocular movements, or the meaning of any sensation connected therewith, from any knowledge of its relation to tactual percepts, except from its conjunction with a *sure* tactual adjustment or movement on the one hand and a *sure* movement of objects on the other. That is to say, both tactual and ocular movements get their whole conception from visual construction in so far as they are known to be related to it, and do not determine that construction.

The projection theory has secured several forms of expression, which it will not be necessary to consider. But they are

all attempts to prove a theory of nativism, and avail only to restate the problem which they pretend to solve. The most plausible statement of the doctrine is that the eye projects images or objects into space in the direction which the rays of light enter the eye or are thrown upon the retina. This view can get a mathematical representation according to the laws of optics. But the trouble with all projection theories is that their form of statement implies, at least apparently, that a process of translation is required to effect the result to consciousness, when, as a matter of fact, the whole content of perception, magnitude, distance and uprightness is given without the 'projection' of anything. If the terms localization or reference were employed without implying any conception of translation there would be less inherent difficulty in the theory. But when it seems to involve the idea of 'projection' into space it implies a distinction between objects as known in space and impressions which it has no right to suppose. And it is worse when mysterious central activities are imported for reversing the retinal image, when, as a matter of fact, the very distinction between the impression and the object of consciousness may be an illusion. But inasmuch as it may create confusion to force the problem of idealism upon attention, we may assume that there is an interesting phenomenon requiring explanation, and only protest against the use of central processes which merely repeat the problem at another point and which cannot be verified or suggested by anything except the fact to be explained.

Now when it comes to presenting a positive explanation of upright vision, or of the compensation for the inversion of the retinal image, it will be necessary to avoid misunderstanding of what is meant by 'explanation.' I do not mean to appeal to any known or unknown cerebral functions which involve a peculiar reaction upon impressions, but only to a law of sensorial action traceable in other senses and adapted to the modified condition of vision. This explains the process by assimilating it to a known and supposably understood process. "In science," says Professor Le Conte very pertinently, "what we mean by an explanation is a reducing of the phenomena in question to a law which includes many other phenomena, and especially the most

common and familiar phenomena."¹ This statement was made in a short article or discussion endeavoring to explain the very phenomena here occupying our attention, but I question whether many of Professor Le Conte's readers realized fully the value and importance of his conception thus formulated. But explanation by a more widely recognized law than the fact in question is distinct in its nature from explanation by an antecedent fact or process, and is the proper resource in the problem of upright vision, because it is the only one capable of direct verification. It will be best, however, to approach my own way of stating the case through that of Professor Le Conte.

In his work on *Vision* some years ago he stated his theory, and more recently in *Science*, where he gives a good diagram in illustration of the process, which may be repeated here.

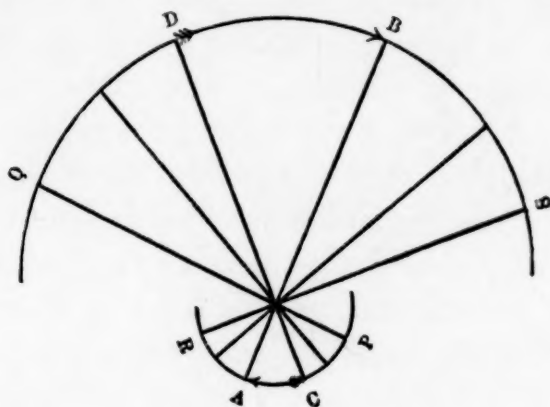


Fig. I.

Prof. Le Conte's explanation of the process is that we refer objects back along the ray lines of light, and "thus the external image is *reinverted in the act of external reference*." The figure brings out this conception by showing that the light from *B* falls upon *A* and is referred back to the point *B*, and the light from *D* falls upon *C* and is referred back to *D*. And it is the same with all other points, *Q* and *S* and *P* and *R*. In his work on *Vision*,² Professor Le Conte gives the same explana-

¹*Science*. New Series, Vol. II., p. 629.

²Le Conte. *Vision*. First Edition, p 83.

tion of the phenomenon, though without any accompanying diagram, as above. After stating the problem, he says: "The true scientific answer is found in what is called the '*law of visible direction*.' This law may be stated thus: *When the rays from any radiant strike the retina the impression is referred back along the ray-line (central ray of the pencil) into space and therefore to its proper place.*"

Before correcting certain misimpressions which I conceive belongs to this way of stating the case, I must mention a personal matter. For a number of years I had supposed this explanation of the phenomenon, though getting very near the solution of it, to be wrong and different from my own, which I had been presenting to my classes. But a year or two ago I had occasion to correspond with Professor Le Conte on this and some experiments in binocular vision, and I found that our conception of the process was essentially the same, and that I had been led astray by his language in the case, which implies a coincidence between the ray and reference line, that appears to be functional, while I wished to separate them functionally, though they might actually coincide. What I shall have to say of his theory, therefore, will be to correct the misconception to which I think it is liable, rather than to object to its real conception and intention.

The objections, therefore, which may be made to Professor Le Conte's formula of the law and mode of illustration are the following: (1) The illustration in Figure I. seems to imply that the horopter or points from which the light comes represent a curved line more or less symmetrical with the retina. This may be a very good theoretical construction of the case, but the same result would hold with a straight line, and this fact requires to be kept in mind. (2) In discussing the theory Professor Le Conte often speaks of 'projection' of the image into space, a form of expression which is misleading, because it assumes space as given and the 'projection' of the point or object into it, as if the percept of space itself were not a mental act essentially a part of the 'projection.' There will be no objection to the language provided we understand this fact. But it is certain that the space percept is an integral part of the total

visual impression, not an independently given datum for consciousness into which it may either 'project' or refer objects. (3) His statement of the law implies a coincidence between the reference line and the ray line of light, and most persons reading it would infer that they are essentially connected, and that a variation of the ray line would be accompanied by a corresponding variation of the reference line. This organic connection I mean to deny, except in so far as evolution may have established an *actual* coincidence which is not necessarily *functional*, and I was glad to discover by my correspondence with Professor Le Conte that we agreed in our conception of the process. Quite a number of his own experiments in the work on *vision*, as well as his comparison of the several senses on the law of direction, establishes this separation between the actual and the functional coincidence in normal vision, that is, between the reference and ray lines, though it is concealed in the formula for stating the law in vision. Hence my own formulation of the process in erect vision is designed to keep this distinction clear, and thus to remove the misconception to which I think Professor Le Conte's language is exposed.

The following Fig. II will illustrate the process for normal vision. The light from *P* falls on all parts of the pupil and is

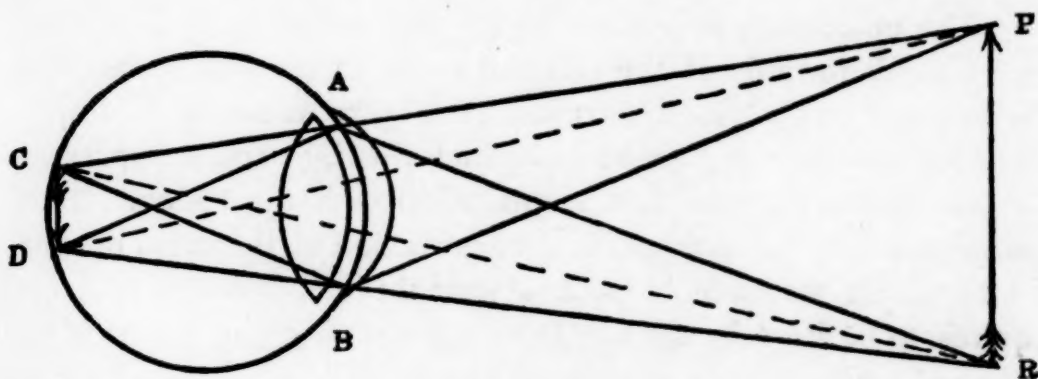


Fig. II.

refracted to the point *D*; from *R* to the point *C*. *PAD* represents the direction from *P* for the ray falling on the upper limits of the line, *PBD* the ray on the lower limits. Similarly for *RAC* and *RBC*. Now either we cannot speak of a single ray

line from any given point such as P and R , or we should have to call it the average direction of such a ray line. In the former case, which represents the facts, there being any number of ray lines with different degrees of angular incidence between the two extremes AD and BD , the reference line PD cannot coincide with them without multiplying the number of objects to be seen. The law of retinal points determines this, and we simply construct the reference line as determined by this fact and without relation to the ray line. In the second alternative not only is the conception of a ray line arbitrary, but there is no evidence of its coincidence with the reference line PD . I would therefore formulate the law of direction without any reference to the ray line, but only in reference to the function of the retina. If we observe the direction of the reference line in Fig. II. and its relation to the retina we can note that it is at least approximately vertical to the surface on which the light falls. Consequently I shall formulate the law of vision upon the basis of this fact. *The law of visual direction or reference is that it is in a line that is vertical to the surface, or point upon which light falls.* This statement implies neither coincidence nor variation from the ray line of light, but it expresses a real or supposed law of the retinal sensorium. Whether the reference line is really or only approximately vertical to the retina it is not necessary to determine. This would have to be a subject for mathematical calculation. But I may state the theoretical form for definiteness and leave any variations from it to be explained by the appropriate causes. It is certain that the reference line sustains actually or approximately the relation to the retina that I have given it, and it remains to give other evidence than the phenomena of normal vision, that will show at the same time both the separation of the reference and the ray lines and the fixity of the reference line, which will be found to be vertical as indicated.

The simple phenomena that prove the law as here formulated are those of phosphenes and Purkinje's experiment. The first of these are produced by the familiar experiment of pushing with the finger against the eye-ball on either side, or above or below, with the eye closed. The bright circles of light thus

produced are not referred in the direction of the pressure, but in the very opposite direction, at least apparently vertical to the point of the impression. In Purkinje's experiment a pencil of bright light may be thrown upon the sclerotic coat at the side of the pupil by means of a microscope, and if the instrument be very lightly shaken, the light that passes through the translucent membrane falls on the retina without refraction, and the shaking of it gives rise to after images caused by retinal shadows of the blood vessels either on the retina or in the sclerotic coat, and these when seen are referred, not in the direction of the light, but directly in front of us in the background at which we are looking. The effect of the experiment is illustrated by the following diagram, (Fig. III.) :

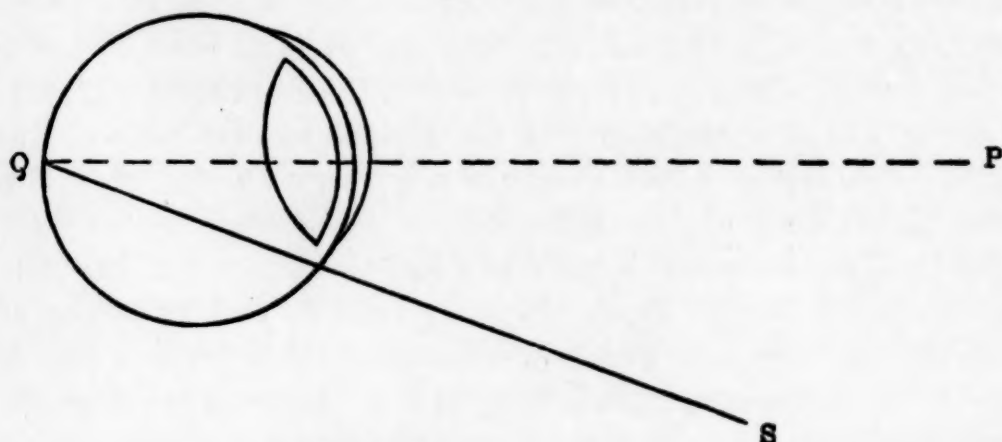


Fig. III.

The line SQ represents the direction of the light and the line PQ the reference line of vision for the retinal shadows. What is shown by it is the fact that the reference and ray lines do not functionally coincide, and that the reference line is vertical to the surface of incidence for the light. That is to say, the reference line for vision is an organically fixed function of the retina, which has no necessary relation to the direction from which the stimulus comes. Professor Le Conte ought to have remarked this fact when he described these very phenomena in his book, and it might have saved the misconception likely to be enacted by his statement and diagrammatic representation of the law. He might have observed this separation of the two

lines very clearly in the experiment regarding retinal shadows, in which he says that 'while retinal images are inverted, retinal shadows are erect,' but are seen inverted, the direction of the light, or rather shadow, that causes them not having anything to do with their reference. As the experiment showing this is important for the theory here advanced it may be quoted: "Make a pin-hole in a card, and holding the card at four or five inches distant against the sky above the right eye with the left eye shut, bring the pin-head very near to the open eye, so that it touches the lashes and in the line of sight; a perfect *inverted* image of the pin-head will be seen in the pin-hole."¹ The pin creates a shadow on the pupils and lens, and *shadows are not refracted*. Hence their images are erect on the retina, their line of incidence being perfectly straight toward the retina, and the reference actually giving an inverted object of sight, while the lines of incidence and reference cannot possibly coincide. The same fact is shown in the illusion caused by the refraction of light in passing through 'the watery meniscus between the two lids and the surface of the cornea.' These are all Professor Le Conte's experiments,² and they simply attest the uniformity of the reference line regardless of the ray or incidence line, and this reference appears in the direction of a vertical to the point upon which the light or image falls. It will be apparent from this how it compensates for the refraction of light and the inversion of the image in normal vision.

Now whether in normal conditions, as represented in Fig. II., the ray line and reference line ever actually coincide, assuming the mean of all that issue from the same point as the ray line, must be decided by mathematical calculation. Evolution may have adjusted the retina with its reference lines to the ray line after refraction, but not because this adjustment is necessary to erect vision. But we must not mistake any real or supposed adjustment for other purposes as evidence of functional connection, because it is easy to show that visual reference has functional stability while the incidence of stimulus varies in all sorts of directions.

¹ Le Conte, *Vision*, First Edition, p. 86.

² Le Conte, *Vision*, First Edition, p. 88. *Philosophical Magazine*, Vol. LXI., p. 266, 1871. *Science*, New Series, Vol. II., p. 667.

We have seen now how erect vision is possible in spite of inverted images, and it remains to show that the reference line is only an illustration of the same law in the other senses. Professor Le Conte remarks this fact and describes it so fully and clearly that I need only refer readers to his work. But I may observe that it is only an illustration of the law of 'eccentric projection,' which is used to describe the tendency of touch to refer a stimulus in a direction vertical to the sensorium or point of contact. This may be a theoretical way of putting the case, but it expresses substantially the direction of reference when a stimulus impresses the sensorium. Now accepting Mr. Spencer's conclusion that the sense of touch was the original germ out of which all the senses were developed, among them sight, we can readily see that nature had only to give the retina a curved form, circular, elliptical, or parabolic, in order to adjust the law of 'eccentric projection' to the modified conditions of vision involving refraction of light and inverted images. The law of direction is thus the same for all sensory impressions, though it is undoubtedly vague in smell and greatly influenced by association and experience in hearing. But in the tactual, thermal and visual senses it is very clearly the same. It will be especially interesting to remark here that, with this explanation of upright vision, we at last secure a direct analogy with touch. The assumption, which we said was not absolutely necessary, turns out at last, under a modified form, to represent a connection which, whether necessary or not, is a fact. The analogy with touch, however, which we criticized, was not based upon the conception of the law of direction or eccentric projection, but upon theoretical conceptions about visual impressions, before it was known that there was an inverted retinal image, and these conceptions happened to coincide with the notion of tactual impressions. But with the law of eccentric projection in touch and the adaptation of the retina to compensate for refraction and the inversion of the retinal image, we have the analogy restored and the phenomenon of erect vision explained by a wider law of sense perception which, in the other senses, presents nothing anomalous.

It will be apparent from the law of visual reference thus

established that, other things remaining the same, if the retina were a plane or a convex surface, instead of concave, objects would appear inverted. Thus in Fig. II. if the retina were a plane surface and the law of direction be as formulated, the point *P* would be seen in a direction vertical to the point *D* in the plane *CD*, and so be located or referred to a position somewhere between *P* and *R*. If the surface were convex, objects might appear both inverted and magnified. All this, of course, assumes that nature might not make compensating adjustments for such surfaces, and only serves theoretically to show that with the law of direction there is nothing really anomalous in erect vision with inverted images, but that the anomaly would exist in the truth of the experiential theory that tactual and muscular sensations correct the original perceptions of sight. Erect vision with inverted images is the natural and proper thing, while any conclusion that the inversion of the retinal image is not necessary to the present result would contradict the law of direction, and simply create instead of solve a problem. Erect vision is simply a fixed function of the eye, just as is that of corresponding points, and we have only to use this law of reference or direction, as Professor Le Conte does, in order to explain single vision by corresponding points.

There is indirect evidence of natural functions for upright vision in the uniformity of its occurrence and the impossibility of securing pathological cases of real inversion that will bear scientific investigation. But if the theory that the inversion of the retinal image is not necessary to normal vision, and that the impression is derived from tactual experience be true, there ought to be frequent illusions even in normal sight in regard to the position of objects. People's experience varies, and we find in all other fields that the products of experience alone show such a variety of conceptions and opinions contradicting each other that it is the sure origin of illusion and no uniformity of results accompanies it. But there is such absolute agreement in human experience about the fact of erect vision that the presumption in favor of its being a normal and necessary function connected with inverted images ought to be as strong as the belief that perception of any kind is a native function of the retina.

This uniformity is even so great that we cannot be sure about the genuineness of pathological cases which either elude authentication altogether or must stand the ordeal of explanation by illusion of judgment. Moreover, we can raise the serious question whether it would even be possible to prove a case of inverted reference, normal or pathological, when reported. Such a result or experience will depend either upon a comparison with memory images or upon a comparison of simultaneous impressions with each other and with memory images, and here the question of illusions of judgment would arise as a ghost to be laid before we could form any opinion as to real inversion. But, however, this may be the uniformity and universality of human experience in respect to erect vision, attests an organic function for sight which is not consistent with the suppositions of mere experience in touch and muscular adjustment, nor with the liability to illusion in the variability involved in all products of experience. The only way to gain support for possible differences of experience in the matter is to raise the question as to our knowledge that others do not see things the inverse of our own perception. But granting that they may do so, if it is as uniform in their experience as ours, and if they experience an inversion of impressions, whenever we do under artificial conditions we have the same evidence of organic fixity of functions in this case as ours, and nativism stands as against empiricism, while to suppose that there is no inversion between the retinal image and real or apparent objects in such imaginary cases of difference between ourselves and others, is to suppose that the same physical structure and conditions of the eyes are not followed by the same optical laws of refraction. Assuming these laws, however, we should have the relation of inversion between image and object, even if we supposed that the image is erect, that is, in the same relative position to objects as seen by another, but inverted in relation to objects as seen by the subject. We should still have both nativism and the law of vertical reference in such cases. It only shows again, however, that the terms 'upright' and 'erect' create illusions as to the nature of the problem and that we conceive it rightly only when we substitute for them the mere idea of an inverted relation be-

tween image and object. In this way tactual conceptions and associations are easily excluded.

But the question of nativism is not the main or first one to be considered. We may view it either as the consequence of the explanation here advanced or as confirming it if independent evidence of nativism be accessible. The real question is regarding the law of normal vision, which will explain the perception of erect objects when retinal images are inverted. This we found to be vertical reference or eccentric projection from the plane or surface of incidence for light. This fact, if it be a fact, shows that inversion of images is necessary to normal visions, and that all experiments to test its nativity by tactual and muscular adjustment are based upon an illusion as to what the problem really is.

THE STAGES OF KNOWLEDGE.

BY PROF. ALFRED H. LLOYD.

University of Michigan.

Knowledge, we are told, is a growth. In conscious experience is to be seen a process that is resolvable into a number of stages. In fact in no science has the evolutionary idea taken a stronger hold than in psychology.

The stages of knowledge, as commonly understood, are four, if I may reduce them somewhat. Thus: sensation, perception, conception and intuition. But an evolutionary series of this kind, however well it may explain its special phenomena, is really in need of being explained itself. Mayhap it is in its entirety, in its earlier as well as in its later parts, a result of the very evolution that it would explain, and if so, the science depending upon it can hardly be too quick or too thorough in recognizing the fact. Mayhap, I say, but in truth I think the supposition is a fair report of reality, and I think also that the science of psychology to-day needs to be brought to a clearer consciousness of itself in this particular respect. Hence the simple suggestions that follow in this paper. I would reflect in a perfectly general way upon the evolutionary series in the growth of knowledge, and expose it, and in the end indicate what seems to me the meaning of the change in psychology that the *exposé* effects.

Psychologists have said of sensation, as the first stage of knowledge, that it is of the material or the physical, of the visible and the tangible and the audible and the like, being a consciousness of what the self is supposed distinctly not to be. They have said that it has for its content only the here and the now and the this, or, in other words, some particular thing in some particular place at some particular time. But the purely passive experience required by this idea of sensation, whereby, to use an old-time distinction, sensation has contained no

thought, no universalizing or generalizing tendency, would have to be in unconsciousness; it would be, after all is said, only a hypothetical stage of conscious experience; as soon as it were realized, the account of it would cease to be adequate. So true is this that more recently we have found sensation commonly referred to, not as consciousness or an element of consciousness at all, but merely as an antecedent of actual consciousness, that is, either the external stimulus or the internal possibility. Sensation has come to play the part of a sort of zero of knowledge, a lower 'limit' in the evolutionary scale.

• Very simple experiments have demonstrated the impossibility of conscious 'sensuous' experience without the quality or meaning of the object being determined by relations beyond its position in space and beyond the moment of its being experienced and beyond individuality or isolation in general. In consciousness, however simple, say of mere color or of pressure or of temperature, other things and other times and other places determine the character of this thing here and now; any simplest object as it enters consciousness gets outwardly reaching relations. The later idea of sensation, then, only marks a retreat in psychological doctrine before this now undisputed law of relativity. But, alas! even science can make the mistake of jumping from the frying pan into the fire, since sensation as outer stimulus or as inner possibility is, if possible, more objectionable than sensation as physical atom or element. Knowledge at zero brings fatal difficulties.

Thus the idea of sensation as stimulus only shows an attempt, very common and doubtless very natural in human thought, to keep the same relations or conditions in unconsciousness that are observed in consciousness. Sensation as stimulus carries the dualism of mind and matter or subject and object into the sphere of life that lies wholly beneath or back of conscious experience. But doing this it robs dualism of all real meaning or content. It makes dualism absolutely formal, a mere hypothesis based upon a questionable analogy. From the point of view of what is without to the conscious self, that is, from the point of view of the object, the psychologist who entertains the notion of an wholly independent stimulus to consciousness

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undertakes to present a stage or a state of life in which the distinction between what is without and what is within is quite impossible. That distinction is a development, not an antecedent condition.

The justice of this criticism is evident also from the complimentary idea of sensation as the subjective basis of possibility, that is, as a 'sensation continuum' or an originally wholly undifferentiated consciousness, a sensuous consciousness of no distinctions, whether in its object or in the organism, out of which a highly differentiated experience with organism to correspond is evolved. But surely one does not need glasses to see that 'sensation continuum' not only is but another name for unconsciousness, but also, like sensation as stimulus, is a sort of indirect or would-be dualistic account of a condition in which mind and matter or subject and object are really not two but one. At least the only object to which it can claim any right is as thin a ghost as ever crossed the path of science. Sensation, then, whether as objectively a mere stimulus of consciousness or as subjectively a continuous or undifferentiated consciousness, in so far as to be regarded the first stage of knowledge, bears witness to an original state of unity or identity between self and not-self.

It sums up the foregoing to say that 'sensation continuum' and sensation as physical stimulus are, in the first place, opposites or extremes that meet, since the dualism on which their opposition depends cannot stand, and, in the second place, purely formal ideas, descriptive of the beginning of mental life only analogically and *retrospectively*. If you must recognize them call them limits, since, as already suggested, they show knowledge at zero; call them abstract limits, but remember that the idea of a limit always gives reality rather to a law operating within the observed members of a series than to a separate thing or a separate state. A limit never is; only the series and its law are; the limit, so to speak, only sets the law and so deepens the reality of each member of the series by making the series itself an organized whole. In mathematics the infinite and the infinitesimal are not real as quantities; they are real only as relationship within quantity or among quantities; they are quantitative indirections for quality and law. So, again, in

psychology, sensation, at best only the infinitesimal of knowledge, whether as continuum or as stimulus, can not be real as a separate antecedent stage of knowledge; it must be, on the contrary, the epistemologist's indirection, or apology, for something deeper than mere knowledge and its stages, say for the vital principle itself or for the impulse to self-expression. The epistemologist has been ready enough to be an evolutionist, but he has also insisted on abstracting the knowing or merely cognitive self. Hence his notion of sensation as a first stage, when in reality it is not that. Psychology, in short, is more than epistemology; it is biology also; and sensation, as evolutionary epistemology has reported it, is, I repeat, an indirection or abstraction for the principle of vital spontaneity.

Just what this principle is, in what terms it must define itself to the new psychology, the foregoing has all but indicated. Thus the law or principle to which the limits, sensation as mere stimulus and sensation continuum, testify or give reality, can have, I think, no better elementary statement than this, namely, that external stimulus and internal motive are one and identical;¹ and in this identity, which makes knowledge from the start, not a mere consciousness of an outer world, but at once a part of and a means to active self-expression, in brief a something on which depends the freedom of the vital impulse, in this identity lies the first law of knowledge, a law which I would have psychology substitute for its still lingering doctrine of sensation as the first stage and the infinitesimal of knowledge. As a law of knowledge it is, of course, a key to the understanding of positive or conscious sensation, which is obviously quite distinct from sensation as mere stimulus, and of perception and conception and intuition, and to the understanding also of the order that psychology has come to give to them in the growth of knowledge.

Positive or conscious sensation, to which I now turn, involves the reference of some so-called sensuous quality to some external object. In different senses the degree of this objective refer-

¹ Certainly, quite apart from what has been said here, one must indeed have difficulty in entertaining the idea of a stimulus so external as not to be also motive or of a motive so internal as not to be also stimulus.

ence varies. For example, it is commonly much greater in the experiences of the eye than in those of the organs of smell or taste, but differences in degree in no wise change the fact or principle, which is our present concern. Conscious sensation is never purely subjective. Psychologists are now well agreed on this point. The experiments alone, already referred to, which have demonstrated the absolute dependence of any sensation for its meaning or quality upon other experiences beyond its own time and place, have left them in no doubt. Some have even imagined that the relationship between different mental states could be mathematically determined; others refuse to go so far; but all accept the general law of relativity. Thus to give Höffding's very conservative wording of the law: "From the moment of its first coming into being the existence and properties of a sensation are determined by its relation to other sensations." That this is a law of the objectivity as well as of the relativity of all conscious sensation hardly needs to be indicated, but, to be perfectly explicit, I may add that 'relation to other sensations' must mean to other sensations in space as well as in time or that dependence of any particular experience for its own special meaning on other moments is also necessarily dependence on other places or positions. If the time relations tend to keep the meaning of the experience subjective, the space relations must make it objective. In other words, the simple law of relativity must be taken as signifying that all conscious sensation is of a spatially external object, or not-self, but of an object whose qualities are in a certain interesting way subjective, in that they must embody by implication, if not directly, the self's past.

But this is not final; for, while the sensuously qualified object, or not-self, must, in the light of the law of relativity, be thought as the past self objectively present to the self, or as the self's objectified past, yet it is clear that the very fact of presentation or objectification points to some change in that past, say to some operation therein performed upon it, and an understanding of exactly what this operation is, is all-important. To get such understanding, however, we must, if possible, get back of the law of relativity or objectivity; we must get back of this law, at least so

far as our comprehension of it depends on an application of its principle merely to differences of position in space and time or even of stimuli of measurable quantity or intensity. Relationship, let us reflect, is as much a matter of unity as of difference, of continuity as of isolation, of organic movement as of external ratios, and the objective world must in general owe its reality to something even deeper than 'difference thresholds' or 'threshold values' or than mere quantities of any kind.

The real difficulty is that so far we have been looking rather to what the sensuous object is than to how it is. The law of relativity, as sketched above, binds all experiences into one differentiated whole, and with space and time as the distinct but inseparable bases of the relations a past self as that which qualifies and a not-self as that which is qualified get their definite places in a theory of knowledge, and are seen of course to have most intimate connection each with the other; but assuredly more must be known than the formal bases of the relations. We must get at the living reality, at the vital principle of relationship instead of its formal bases. The theory of knowledge has been tarrying too long at the sign of the formal bases.

And what is the living reality, the vital principle? Plainly it is the impulse to self-expression with its identical aspect of motive and stimulus. Impulse to self-expression, at once differentiating and organizing, since both a wholly random act, an act in general, and a wholly specific act, an act in isolation, are not only unknown but also unthinkable, is a principle that quite transcends the special forms, space and time,¹ and yet that, so far as manifesting itself in motion, is spatially and temporally interpretable. Action, identical with self-expression being neither random nor specific, neither general nor individual, must be as expressive of control as of spontaneity. Accompanying all action, then, there must be a tension, or, the same thing, a consciousness, consciousness being always a tension between control and impulse or spontaneity. Control, however, is just that aspect of self-expression which gives reality to a not-self

¹ One has here to think of Spinoza's self-caused and self-intelligible substance, existing *in se* and *per se*, that transcended thought, the time aspect of law, and extension, the space aspect of law, in that it had infinite other attributes. Spinoza, be it remembered, all but gave Leibnitz his self-acting monad.

with stimulating qualities, while spontaneity, as a positive impulse in tension with control, shows these qualities to be real only as embodying the past, since such qualities must be reminiscent. In short, the not-self, as qualified, does but show the self's past in tension, and whether we approach the matter from the standpoint of self-control or from that of the not-self in whose qualities the self's past is seen to be in tension, the presence of a process of organization, of an organic activity, is beyond question,¹ and the organization plainly is not less of the objective qualities than of the controlled activities or impulses belonging to them. But organization of any particular impulse signifies reduction of it to a pure means wholly adapted to the single end of the organism, or to such a condition that when expressed the whole self can identify itself with it; and organization of all the manifold impulses must signify the development of activities every one of which can and must fully express the self. And on the side of the objective qualities, often called the outer stimuli, a perfectly correlative process, as already said, must take place, in that each individual quality, or sensuous content, corresponding to an individual impulse, must in its function of stimulus appeal to the whole self, not to the impulse as an isolated activity; and must therefore in its qualitative character be determined by a relation to the other qualities. Each qualitative part, or content, of the not-self, in so far as stimulus, must be at the same time, by 'fusion,' if you will, or 'assimilation' or 'association,' the qualitative whole, and just herein we see from the standpoint of impulse to self-expression, which is in action a process at once of differentiation and of organization; just herein do we see what the law of relativity means, how it is a law of objectivity, that is, a principle of control, and in being this is also a law of organic activity, and how, secondly, from the same standpoint, stimulus just in so far as it produces 'reaction' must be absolutely identical with motive.

¹ Compare with this account of the origin of the not-self the extremely interesting special case of it in the generally accepted explanation of the idea of space as rising with organization and symbolization (through association of muscular and tactual and retinal sensations) of the motor-impulses. Space as geometrical—that is, as mathematically definable—is the objective correlate of free movement. An exact geometry is possible, 'innate,' only to such as have the power of free movement.

The identity of motive and stimulus, above suggested as the first law of knowledge, very materially modifies a current idea of 'reaction,' as but just now hinted, and gives a notion of environment, heretofore styled the not-self, that has far-reaching consequences alike in psychological and in ethical theory. To add a few words upon the first point, that of the proper conception of reaction, it is evident that identifying stimulus and motive reduces reaction wholly to a process of the self acting upon itself or within itself, or even of environment acting upon itself or within itself, and not to what has been so often assumed, a process of self or mind, as something essentially distinct in nature, acting or reacting in its own peculiar way upon not-self or matter. Indeed, 'reaction' is a term imbued altogether too much with the spirit of dualism to be at all safe in self-controlled discourse. 'Self-activity' is far better, and with the conception of environment here required, whether one means self-activity of environment or self-activity of self is of no importance. "Environment did it" equals "self did it" in all cases of action, since the stimulating quality by reason of its determining relations is environment as a whole and the organically qualified environment as a stimulating whole is one and the same with the organic self and its impulse to complete self-expression.¹

The notion of environment that our law of knowledge enforces is this: Environment is the self present to itself, in an other-than-it form, the otherness always signifying a tension between control and impulse to act. So much was really implied in what was said in a former paragraph, namely, that the not-self must be regarded as the past self presented to the self, or the self's objectified past, or with regard to its stimulating qualities as the past in tension; but now we have clearly before us the operation performed upon that past, as implied in the very fact of the presentation or the objectification, this operation being nothing more nor less than organization into the present or ad-

¹Quite another way, and to me a very suggestive way, of putting the foregoing is that the human body is to be looked upon only as a part of 'environment.' The tendency to cherish it, whether in the passing psychology of reaction or in religion—of course the two are related—must give a false idea of activity. In reality the human body is but a part of a physical whole; it is, however, a part whose activity is that of the whole; hence it has mind or soul.

justment to the present, so that environment or not-self proves to be the past made present, its qualification being always a process of adjustment to the present. In other words, environment as differentially qualified and self as organically free and active develop together, not, as some have seemed to think, does environment with stimulating quality exist before a living self with interest in it, nor, as others have maintained, does the living self or soul antedate its environment. God did not *create* the world nor is man in any sense so creative, nor on the other hand is man in the ordinary understanding of the doctrine evolved out of the physical. Both the physical with its qualities, primary and secondary, and the freedom of self expression are evolved together. Evolution of course has been disposed to put the qualified world first in time, and creationism to put the fully developed spirit first in time, and which has committed the grossest anachronism it is really hard to say, since to reiterate, neither came first, or rather both were first and have kept the contemporaneity from the beginning. Thus, to suggest large-written illustrations, I can imagine man, when first assuming the erect position congratulating himself on having relegated so much of his past as was in the going on all fours to a mere object or symbol in his consciousness, and I can even feel the interest he must have taken in the new qualities and the more organic character that his world came to have for him with the change. The wanderer returning after long years to the scenes of childhood and seeing as object with emotional qualities that in which he had once lived, with which he had once wholly identified himself, could sympathize too. But how absurd it would be for either the returned wanderer or the erect man to say with materialistic evolution that out of that object *as so qualified* he had been evolved, or with orthodox creationism that in the object there was evidence of a fall from an ideal state to which, however, he has now at last returned. And yet upon such abstractions, upon such anachronisms, even recent psychological doctrine, in its idea of reaction and in its idea of the stimulating medium very largely relies. True, the past is in the object, or the environment, or the stimulating medium, but because the object is, and is at once organic and relationally differentiated, the

present is there too, and if the present also the future. The object is not reminiscent merely; in being object it belongs to the present; it is, again, in so far as qualified and in so far as constructed or ordered literally 'up to date;' it is a stimulus that is also motive; it is a revelation as well as a reminiscence, the future as well as the past.

In environment, or object, as now present to our thinking, we have of course the 'perceived world.' The study of positive or conscious sensation has brought us to a comprehension of what psychology knows as perception, the second stage of knowledge. The law of relativity, as also a law of objectivity and of organic self-expression, under which a sensuous consciousness develops into a consciousness of an ordered outer world present to a self-controlled self, under which the world of experience becomes in the technical sense a 'perceived' world, carries with it, as we have found, three things: (1) the persistence of the past or of past experience in any present consciousness, (2) a differential, or negative, qualification of consciousness, by which the objective reference springs up, and (3) an organic activity, whereby consciousness, becoming objective, gets what is commonly called symbolic character, being symbolic of the activity itself. Perception, then, is a process by which the past may be said to move over into the object and to abide there as an important phase of the present, and the percept, the self so present to itself, is the original unity of the self as an organism differentiated and in the differentiation projected as not-self. The percept, accordingly, is not-self, but very much as the band of prismatic colors is the not-self of unresolved light, or as tools and shop and materials are the not-self of the tool-using mechanic, or finally as a social community, in which labor is divided, is not-self to each one of its members. The percept is not-self, but also the incarnate self. What else can its tendency to symbolic character signify? Now perception, as the perceived image evolves into mere symbol, becomes conception and intuition. This is technical and abstract. But the meaning of images becoming mere symbols is not far to seek.

Any image in which by virtue of its being objective and

of its sensuous qualities being organized, the past is fully adjusted to the present is become a symbol; it is a *mere* symbol. With the perfect adjustment the image's or object's qualities lose their reminiscent and individually stimulating character; the 'associated' past, on which the consciousness depends, comes to be so real in the present as no longer to be suggestive of the past; whereupon the reminiscent qualities lose value, except that of the relation embodied in them, and the object as a mere system of relations, an organically relational whole, becomes not an object of sensuous consciousness, but a symbol, that is, a *basis of activity*, and has the same relation to the life of the self in general that language with its 'parts of speech' has to the expression of thought. In an image or object or symbol so developed, the self is set free. No symbol is mere symbol that is not proved so by some action in use of it, and the action of course fulfills motive and stimulus as one.

The term language, so says this psychology, must be extended to include the object of consciousness in this sense of the used symbol. Indeed one has to think of parts of fluent activity in general instead of merely of parts of speech. In parts of fluent activity psychology sees the survival of the sensuously stimulating qualities or elements which in the evolution of experience gradually pass into mere terms in a system of relations. Indeed, in the narrow sense, what is language, if not a complex of 'dying metaphors' or 'material associations,' or 'passing reminiscences', dying or associated or passing in the interest of organization or adjustment or fluency? Yes, the whole world of perception, as it becomes symbolic, as in it the past is adjusted to the present, is essentially linguistic, the basis of fluency in action; it is language that the perceiving self can use, with this limitation that when the self uses it as language, when the self acts fluently in it instead of simply observing it, it is more properly called the world of conception, since the self is then rather conceiving than perceiving reality.

It is common enough in psychology to connect intimately conception and the use of language, but observe that here, as the term language is made to include so much more than it usually covers, there is demand also for a more inclusive idea of

conceptual thought. All sensuous images becoming symbols are linguistic; all fluent expression of self, all free activity, is thought in the stage of conception. Language obviously is only another name for the not-self as the self incarnate.¹

But to some I shall doubtless seem to be denying the very most essential function of language, which is to name or report or describe. Language, I am reminded, enables its user to stand aloof from the physical world and to carry on an abstract activity—with reference to the world, it is true, but quite apart from it. So separate from ordinary activity has language been regarded that it has even been declared to be a gift of heaven, not of earth, an integral part of man's spiritual equipment. But let me say, varying a little what has been said already, that all free fluent activity is abstract or separate in exactly the sense meant. Finding a use of language in all fluent activity is not at all opposed to the orthodox ideas of language. Forsooth, are writing and speaking the only cases of self-activity? Every spontaneous act, every expression of the living self shows, in the first place, an experience organized into a symbol or a past brought into adjustment with a present, and in the second place this symbol as something belonging to the active self, something which mediates the activity, something quite as much motive as stimulus, applied as a 'name,' or a 'report,' if you like, to an outer world. All action, I would assert, from the lowest to the highest, from the simplest to the most complex, is of a self *naming* a not-self. When action is, the object or the symbol is as much the agent as the subject. So, in the sense of language being original, one cannot object to thinking of it even as a gift from heaven; it is as original as activity. Biologically, freedom in an environment is also freedom of an environment, and organism and environment are one as thinking-self and language are one.

It has not infrequently been a matter of controversy if thought were possible without language, and the solution of the problem seems to be that thought is possible without language

¹ On the more general use of the term language compare the short discussion: 'A Psychological Interpretation of Certain Doctrines in Formal Logic.' *PSYCH. REV.*, Vol. III., No. 4, July, 1896—pp. 422-426.

in the narrower sense, that is, written and spoken language, but impossible without language as such, that is, without environment. Do animals think? Do they know relations? Are they addicted to language? Assuredly they are and do, if ever they act in self-expression. What living creature is not 'addicted' to an environment?

The rise of language as such, so our psychology here would lead us to conclude, means nothing more nor less than the mechanicalization of environment or the perceived world. In other words, the pure symbol, used in conception, in so far as a basis of fluent activity, is only a mechanism that the individual has become free to use. And in such mechanism, I would suggest in passing, as objective or physical, is evidence always of the rise of a community of individuals acting organically. In short, the fully developed object of perception is more than mere symbol; it is a mechanism in which is the basis of the life of a social organism. Its character as language, as 'medium of the exchange of thought,' can have no other import than this, since thought itself is organized social life. This intimate connection, moreover, between the rise of language and conception, the mechanicalization of environment, and the development of the social organism, is a most important outcome of the standpoint taken here, but discussion of it is not within the scope of the present article. Motive and stimulus are identical; environment is essentially linguistic; and language is not the medium of the exchange of abstract thought, but the basis of an organized life. That is the whole story in a nut-shell. Simply the linguistic environment makes possible individual self-expression in a social group; or mechanism, like the *a priori*, which according to Kant makes the experience of it possible, is social.¹

So far I have insisted on extending the use of the term language to make it include environment or the medium of expres-

¹ Social, I repeat, in the sense of free industrialism or of society as an organism; in history the social mechanicalism of Rome is evidently the '*a priori* from' in which modern life as industrial and organically international has been possible. Rome, with her Christian idolatry, her spiritual monarchism, her linguistic formalism, her Jewish finance, only have witness to the *originality of the medium of self-expression*, an idea which the modern individual has naturally enough taken to himself. But, in general, mechanism is the *a priori* condition of individualism and organism.

sion without limitation or abstraction. But, of course, language in the narrower sense, in the sense that limits it to special forms of sounds and shapes, has, even in its very narrowness, an important relation to the activity of thought. In emphasizing the broader view of language, therefore, I have appeared to slight the narrower. Hence I wish to add the following much condensed paragraphs by way of atonement.

It is a generally recognized principle that self-expression brings interpretation or meaning to the impulse expressed, and that meaning, coming so, controls the impulse. In other words, after expression impulse is held for a time, longer or shorter, in abeyance. Impulse in abeyance, however, not only confines the self's activity within the self, but also changes the special centres or organs of consciousness, and the confinement and the change would seem to be what make language in the narrower sense. Thus, to give the most obvious illustration, an impulse of man's in abeyance does not mean inactivity, but activity abstracting itself and identifying itself with eyes and ears, with the writer's hand and the speaker's tongue. Where, indeed, could activity find itself more at home than in these marvellously mobile organs? They are, in fact, but the stage upon which the self rehearses its part. They show the self acting 'to itself,' as we say specially of a child that learns to read without speaking; that is to say, acting apart or abstractly or reflectively.

Control, then, abstracts activity and develops very mobile organs for the special function so arising, the function of acting to oneself or quite within oneself. But acting to oneself brings the consciousness of environment or not-self; and, more than this, the environment gets what, in lack of a better account, I have to call a double character. Thus there arises a special consciousness, or experience, inhering in the special organs of the abstract activity and, at the same time, a special consciousness inhering in the organs of the self's complete activity, and, obviously enough, the special object of the former serves as a name or symbol of the latter; the special object of the organs of activity to oneself *names* the special object of the organs of the possible activity to one's world. The self does not talk talks nor see sights, nor hear sounds; it *names* the not-self.

And, furthermore, in the name, arising as the object or natural medium of the abstracted activity, relationship, that is, relational or organic structure, will far outweigh all consideration of mere size. The name, in short, will be only a sort of after-image of the sphere of the self's original expression of impulse—original, that is, antecedent to the rise of control. That the original expression will have already determined the relations, or given the self an experience of them, is clear enough, since without such determination the expression itself could never have taken place. So the reproduction in an after-image is no miracle, but only shows how realized relationship in experience brings independence of mere quantitative determinations. The theory of language as originating in pictures, reduced reproductions of natural objects, has its limitations, but it will serve here in illustration. Its limitations, after all, are rather in terms of narrow application than of principle involved.

So, in summary, expression of impulse puts impulse in abeyance; impulse in abeyance brings an after-image of the special experience, which, as a relational whole, expression has defined; and, the after-image being a freed image, or the sphere of an abstracted activity, the direct use of it, the use of it with reference to its origin, the controlled, mediated use of it, gives what we commonly understand as the linguistic expression of self.

But we have yet to consider the last stage of knowledge, intuition. Intuition, however, is but the perfect freedom of using language, or of adaptation to environment. It is a stage of knowledge very much as sensation was a stage of knowledge. Thus the used mechanism is, as it were, the limit that the sensuously qualified symbol approaches, and intuition as stage of knowledge is a limit too, being such a limit as we have seen sensation to be and giving evidence of the same law of knowledge. Intuition comes with the completion of the process of mediation; with it consciousness ripens into fluent action; with it thought is set free. If in sensation stimulus and motive are one, in intuition at the other end of the scale developed mechanism as the stimulus and free agent as the motive, in short, language and thought, are one. The mechanism is nothing more nor less than stimulus to the *free* agent's will.

So at both ends of the scale evolutionary psychology has erred. It has retained intuition as the last stage of knowledge with the same blind persistence, or rather with the same misunderstanding of the true meaning, that has characterized its treatment of simple sensations or of sensation as stimulus or original continuum. Neither sensation nor intuition is a content of consciousness. The former is the vital impulse to self-expression; the latter is that impulse fully mediated in an act. As stages of knowledge they are limits, the infinitesimal and the infinite respectively, and, so understood, they only show how psychology, bent on keeping knowledge in a sphere quite by itself, has striven to do without physiology and biology. Thus, again, sensation as stage of knowledge is the back-door by which psychology has spirited life into the domain of knowledge; and intuition in its turn is but an epistemological disguise for the ripened act; and if the former is due to the gratuitous construction of retrospection—on the part, say, of self-conscious inactivity—the latter results from a closely related prospect.

And the change in psychology, finally, that recognition of this origin of the evolutionary stages of knowledge effects is simply the turning of the old-time idea, or concept, into an act; of self-conscious inactivity into activity; of psychology, science of the soul, into biology, the science of life on earth.

DISCUSSION AND REPORTS.

THE PRESIDENT'S ADDRESS.

In his recently published address (*PSYCHOLOGICAL REVIEW*, January, 1897) Professor Fullerton, after firing random shots at a full score of ancient worthies and modern colleagues, trains the heavy guns of his critical raillery on my views as to the nature of mind. His reiterated charges of 'obscurity' and 'inconsistency,' made in the lightsome mood to which we have all become accustomed, I am entirely content to let stand for what they may seem worth to those who have carefully read my books. I only wish at present to call attention to two or three misapprehensions. Perhaps, however, even this may have some bearing upon the charges if, as it seems to me, the misapprehensions are so obvious and on the surface as to show cause *in the critic*, why he should find the views of nearly every one else, with mine, guilty of essentially the same errors.

In the first place, I am charged with having abandoned the standpoint of psychology, because I have insisted that all the phenomena of consciousness must be considered not merely 'content-wise,' but also 'function-wise,' and indeed as forms of self-activity; and also because my analysis of cognition shows that cognition always implicates reality, 'envisaged, believed in, or inferred.' But I find Professor Fullerton himself, in this very address, insisting upon a 'broad and reasonable sense of the word content,' and affirming by it "*I mean all that is to be found in consciousness, including relations, changes and activities.*" Moreover, he commends Professor Wundt for treating the subject-matter of psychology in the proper way; although he patronizingly adds in a note that Wundt, too, does not appear 'to fully appreciate the significance of his own position.' But does not all the psychological world know that Professor Wundt makes, in his psychological writings throughout, prominent use of the doctrine of the soul as a conscious self-activity, and that concerning the relation of psychology and philosophy he has even expressly denied the possibility of treating them as independent disciplines. As to knowledge, however, in this very paper also Professor Fullerton repeats, with evident increase of self-satisfaction, what he had to say in a paper of three years ago: The psychologist 'must assume (*sic*) the existence of an external physical world,' of which our ideas are copies that are

'intimately related to particular bodily organisms.' Now it seems to me that any clear and consistent thinker will be forced to exclaim over such a tenet as this: Here is 'belief,' and ontological 'implicates' inferred, with a vengeance. Perhaps Professor Fullerton will sometime free, for us all, his little bit of an 'assumption' from the 'obscurity' and 'vagueness' and 'inconsistency' in which he has left it.

But a much more serious and quite indefensible misapprehension seems to me the only explanation of Professor Fullerton's method of criticizing my views by a kind of see-sawing between the two books, 'Psychology, Descriptive, etc.,' and 'Philosophy of Mind.' He is, indeed, so kind as to admit that I am developing these views in 'the right direction.' But curiously enough, the one which is really the earlier of these two works, but which my critic appears to regard as the later, sets the high-water mark of my poor attempts to be clear and consistent, as well as 'learned and really scholarly,' respecting the doctrine of mind. What, however, is the actual case? The first book *is* what its title signifies, namely, an attempt to describe the development of human mental life in the individual; and, among other forms of development, the growth in clearness and complexity of the conception of Self, just as observation and experiment and scientific analysis find it. With my accomplishment of this task Professor Fullerton has little fault to find.¹ But the avowed purpose of the 'Philosophy of Mind' is, without abandoning the standpoint of psychology, but by transcending this standpoint and passing on to the standpoints of metaphysics and epistemology, to give speculative treatment to the phenomena of consciousness. In other words, I have made, in this later work, the effort to construct a rational doctrine of the real nature of mind. Now, like any other critic, Professor Fullerton might deny to me the right or the ability to attempt such a task, or he might refute the positions taken in the course of the attempt. But to overlook the relations of the two works; to cite, as my final view, sentences from the former which I have quoted into the latter so as to furnish my speculation with empirical data; to impart meaning into metaphysical terms which I have most expressly guarded against or even rejected, and thus to throw into confusion and inconsistency what is clear and consistent when read in the connection and in the light of the author's intent—this seems to me a style of criticism which is best left to itself to refute.

One more misapprehension I wish to notice. I am accused of teaching a kind of 'diluted' Kantian doctrine of the soul as *ding-an-sich* or *noumenon*, lying behind all actual self-known existence and answering either to a purely negative and limiting conception or to the

bare idea 'that it is,' without the possibility of knowledge as to 'what it is.' Shades of the great founder of critical agnosticism! And yet I have been studying carefully over and over again the 'Critique of Pure Reason' for years and with scores of keen and critical minds as pupils and co-workers, and have never discovered my agreement on this particular doctrine with the sage of Königsberg. But since I can scarcely ask Professor Fullerton to read again the 'Philosophy of Mind,' where I have, as clearly as language can and so often as really to run great conscious risk of wearying my readers, tested and rejected the Kantian view, I know nothing better to suggest for him than a revised study of Kant. Perhaps this will lead him to discover unlimited chances for obscurity and inconsistency in his own attempt to place a writer who affirms that *we do know reality*, beyond all power of sceptical idealism or agnostic positivism to shake the foundations of such knowledge, in every act of self-knowledge, and that *all knowledge is, quoad knowledge, essentially transcendent*, agree with the great author of the 'Critique of Pure Reason,' who taught on all these points precisely the contrary view.

Much more might be said about Professor Fullerton's manner of treating those whose names and opinions he is wont to handle with such effective appearance of grace and ease. But I prefer to leave sword-play for the most part to men who like it and who really think it leads to truth, and to content myself with the humbler and less impressive use of trowel and spade.

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UPRIGHT VISION AND THE RETINAL IMAGE.

Professor Hyslop's recent objection to my article on 'Vision without Inversion of the Retinal Image,' in the November number of this REVIEW, is a welcome criticism of the bearing of my experiments, even though the form in which he has seen fit to express his objection is, perhaps, needlessly brusque. He says, in substance, that I have missed the real problem of upright vision in taking it as a problem of the harmonious interorganization of motor, tactual and visual experience, and that the real problem is an exclusively visual one. According to his view, the question of upright vision is: How do apparent objects get a spatial position inverse to that which they have in the retinal image? And since my article, to his mind, shows clearly that during the experiment the position of apparent objects was

still the reverse of their position in the retinal image, my experiment has nothing to do with the problem of upright vision.

I am glad that Professor Hyslop has taken the trouble to show that the problem, as I understand it, is quite different from this problem which he counts the true one. The two problems have hardly anything in common, and it is well that everyone should see that mine is not his. For his problem is, I feel sure, an illusory one and vanishes as soon as one sees the true relation which vision, as a whole, bears to the retinal image.

It is sometimes said that one never has an experience of his own retinal image. This is perhaps strictly true, and yet it is often misleading, in that it is understood to mean that the retinal image is outside my experience and yet not so alien, but that somehow I can compare its position with that of my visual experience. The fact is, the retinal image is, by representation, made a part of my experience, just as all things which I represent become thereby parts of my experience, even though I do not directly perceive them. And only by thus representing my retinal image and definitely assigning it a position within the world of things actually visible to me does any comparison of its position with that of other objects become possible. It is visualized, or otherwise represented, in definite spatial relation to those parts of the world which I see, and thus becomes an integral part of my larger world of visual and visualized experience, built out beyond and in between the objects of actual sight. My brain, for instance, becomes a part of my visual world because I assign it a definite position within the visual total, though I have never seen it. I represent my brain, not as enveloping my experience nor as having lines of direction independently comparable with those of my visual world, but as itself a part of that total visual world and as having for me no position nor direction except as within that total and as relative to the other parts of the whole. Its position in my world of experience is nothing absolute, but is determined merely relatively to the internal lines of direction and points of reference of that experience. Likewise my retinal image is an integral part of my visual world. Its place is within my visual total, and its position and direction are determined only by making use of the directions of reference within that total. Why it should have the position and direction there which it does have; in other words, why the rest of my visual world and that small portion of it, which I call my retinal image, should have the peculiar spatial relation they do have, is a matter of optics and vertebrate morphology, not a problem for psychology.

The position which, from our knowledge of optics, we assign the visual image within our visual world does not mean that our visual experience bears an inverse relation to something *external* to that visual experience, as Professor Hyslop seems to think. This relation is in no sense a relation between two heterogeneous terms, one of them a system of visual experiences, and the other an alien counterpart inverse to these. Since our only way of comparing the image with our visual perceptions is by representing it relative to their position, and as embedded in their larger system, its relation to the rest is no indication of the relation of the whole system of visual perceptions, or of the visual process as such, to something else. It does not give us the slightest warrant for holding that the visual process includes, for instance, a process of spatial transposition of objects into some other direction or order than that given in the immediate retinal stimulus. The *interrelation of objects*, not the absolute position of objects, is what we wish to know by sight, as by touch. Even if we could make absolute position at all intelligible, a knowledge of it would be of no earthly use to us, except in so far as it might guide us to a knowledge of the relative situation of things. Now our vision gives us this *interrelation of objects* exactly as their images are interrelated in the retinal image. We see things in the very same relation to our body that the images of those things bear to the image of our body on the retina. Later on, the reflective mind wishes to add into its visual system of objects other objects not given in vision, and among the rest interpolates one small item not appearing among the images on the retina, namely, the retinal image itself. The fact, that I represent among my objects a smaller inverted image of some of them, seems to me no better evidence than, for instance, an inverted chair among my visual objects would be that transposition or reversal takes place in the process of vision itself. If Professor Hyslop really thinks that the position of visual objects with reference to the visual image reveals a peculiar character in the visual process itself, such as to constitute a problem, there must be for him a still more serious problem in the fact that our visual objects appear to be in front of our head, though the real organ of vision is in the occipital cortex.

Vision as a whole and by itself is indeed neither inverted nor upright. Objects *within* the visual system may be inverted or upright with respect to other objects in the system; but the whole cannot by itself have either of these characteristics. For this reason there can never be a purely visual problem of upright vision. And since visual experience cannot be compared with things-in-themselves, nor con-

sequently with the retinal image-in-itself, upright vision must mean a vision which gives us objects upright with reference to some non-visual experiences which are taken, for the time being, as the standard of direction. Upright vision, in the final analysis, is vision in harmony with touch and motor experience; and the only problem of upright vision is one concerning the necessary conditions for a reciprocal harmony in our visual and tactual or motor perceptions.

Now the actual conditions of vision make it seem, to a person who takes an uncritical common-sense view of things, a matter of surprise that there is harmony between these different kinds of perceptions. Since the retinal image of any object lies in a direction inverse to the object as a touch experience, the nervous basis of vision seems to be in discord with the system of tactual perceptions; how does it come about, then, that there is mutual harmony in the two forms of perception? The theories which may be roughly styled the projection and the eye-movement theories answer this question by stating, each for a different reason, that vision reverses the retinal direction of objects. The real visual direction, as distinct from the merely retinal direction, is thus, according to these theories, identical with the touch direction, and the problem is solved. But an implied corollary of either of these theories is, that if the retinal image were *not* inverted with respect to the tactual position of things there *would be* discord between the two kinds of perception. For the same mechanism which hitherto had produced a reversal would remain; the reversal ought, therefore, to take place persistently, and visual objects would in that case be spatially the inverse of their tactual counterparts. These theories tend, therefore, to the result that an inverse relation between tactual direction and the direction of the retinal image is one of the necessary conditions for a harmony between touch and sight.

My experiments make it extremely probable that the harmony rests on no such condition whatever; and this probability is still farther strengthened by later and more extended experiments, of which I hope soon to give a detailed report. Both sets of experiments go to show that when the retinal direction of objects becomes identical with their tactual direction the discord in the experience is only temporary. In fact, the experimental results confirm the truth of the view stated near the beginning of this paper, that we have no reason to suppose that there even is a reversal or transposition of directions in the visual process. *A fortiori*, we need no theory to explain the reversal.

Professor Hyslop, however, points with assurance to certain passages in the report of my preliminary experiment, as proof that such

a reversal was present even under the conditions there described. What I have already said of the relation of visual experience to the retinal image, is, it seems to me, a sufficient answer to his interpretation of the facts. But even from his own point of view the passages he refers to are innocent enough, when one distinguishes carefully between that portion of my experience which was based on the older visual conditions and that portion which was being constructed under the new (experimental) conditions. I stated in my paper that when I artificially turned the retinal image upright I saw things at first upside down. Now, since the retinal image was turned 180° and visual objects, in consequence, were turned 180° , this means to Professor Hyslop that the normal inverse relation between image and objects still held, and that my experiment is only an additional evidence of how persistent this relation is. I admit that in my mixed experience at the beginning of the experiment, and in general throughout the experiment (for the experience to the end was a conflict between old and new), this relation existed. But it existed simply because the experience was a mixture of old and new perceptions, and the directions of reference were largely still the old ones. My 'real' body was, in general, localized as I had seen it in my pre-experimental vision. The retinal image was localized with reference to this older visual position of my body, and not in the way which a complete submission to the new visual experience would have required. As long as my body was localized according to the old experience, and other things in sight were localized according to the new, the two standards for localizing my retinal image were in conflict; so that the image's correct relation to one of these standards meant its inharmonious relation to the other. An entirely harmonious organization of the new experience, based on a full knowledge of the laws of light, would have required that the retinal image should be localized among the objects of my experience, in an upright position with respect both to my body and to the objects represented in the image. But since my body was, in general, still localized by recalling pre-experimental perceptions of it, a localization of the image in proper relation to this old position of the body made the image inverted with respect to the things I saw. And if, on the other hand, I localized the image in proper optical relation to the things it imaged, the relation between the image and my body was incorrect. In general, I no doubt remained faithful to my body and let the outer contradiction take care of itself.

But all this is only a transitional state of consciousness. Suppose

that the partial reharmonization of my experience had given place to a complete harmony of tactual and visual perceptions and to a suppression of my old localizations brought over from the earlier experience—a result toward which the experiments surely point;—I would then feel and see my body unreservedly in its new place in the visual field, and in the same relation to the new objects around my body, as existed between my body and surrounding objects in the older experience, viz., my feet on the ground, my head toward the sky, etc. The proper localization of my retinal image according to the laws of experience, would now produce no such contradiction as was inevitable during the earlier, transition state. I could localize the image—and a self-consistent organization of my new experience would force me to localize it—upright with respect both to my body and to the objects pictured in my image. The inverse relation between my retinal image and the objects perceived would here have disappeared.

The result toward which the experiment points has thus a most definite bearing on the problem of upright vision, even in Professor Hyslop's sense of the term. And instead of adding testimony to the persistence of the inverse relation between image and objects, it really shows that this inverse relation is a psychologically non-significant accompaniment of the peculiar lens-arrangement of the eye, and would disappear could we but change the eye in that regard alone. If our eye had contained a more complex system of lenses instead of the simple arrangement we actually have, there would have been no hint in our experience, and certainly none outside of our experience, of any mutually inverse relation of objects and their retinal images.

Through the courtesy of the editor, I have been permitted to read advance sheets of Professor Hyslop's article in the present number of this REVIEW. The grounds upon which he denies the pertinence of my experiments to the question I had in view are fully covered, it seems to me, by what I have already said. Nor do I see that he has yet produced a single fact to show that the interrelation of visual objects is not *identical* with the interrelation of their retinal stimuli. Since visual objects have no absolute position or direction, but only relative position and direction, there is no evidence that vision reverses or transposes anything, until some one shows that vision gives us objects in some different order or interrelation from that which their images or stimuli have *among themselves* on the retina. Only a reversal of this sort would give us a visual problem. And since no such reversal or transposition occurs, there is no exclusively visual problem of upright vision, as Professor Hyslop supposes.

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GEORGE M. STRATTON.

THE ORIGINALITY OF ÆSTHETIC FEELING.

In the September number of this REVIEW, Dr. Livingston Farrand has deserved well of all interested in the philosophy of art, by calling attention to Grosse's *Anfänge der Kunst*. Agreeing, as I do, with his high estimate of the book, I wish to point out briefly the significance of some of its conclusions. As the title indicates, the author has limited himself to the historical and descriptive treatment of his subject, but his results seem to have some bearing upon the nature of the æsthetic impulse itself. Is the beautiful a variety of the useful? Does it exist in and for itself, or has it an end beyond itself? Can we analyze our feeling for it into yet simpler elements, or is it an immediate and ultimate judgment of value? The bearing of Herr Grosse's work upon these questions is what I wish to discuss in this paper.

The author's conclusions are best considered with reference to the particular divisions of the arts made by him, for the results vary slightly in the different arts. (1) Personal adornment holds the first place in his classification; does this show an immediate feeling for beauty or is it undertaken for ulterior ends? Apparently it serves a two-fold end, that of attraction and that of repulsion. Primitive man adorns himself either to attract his mate or to terrify his enemy. Even the most primitive form of dress seems to have this external end, rather than the more immediate one of serving as a protection from the cold or as a concealment of the person. The main purpose of early adornment was the same as that found in animals, the furtherance of sexual selection. Early art in this most primitive form had thus an important function in the development of the race. It was not a mere accident of evolution, but one of its forces, a means to the survival of the fittest. (2) Again, the ornamentation of weapons and domestic implements in most cases seems to have been undertaken for utilitarian reasons. Their smoothness and polish were a direct advantage to their owner. Moreover, their symmetry and proportion were not necessarily due to æsthetic feeling, but were the result of the inherent possibilities of the instrument itself or due to imitation of nature. The laws of mechanics are accountable for much apparently æsthetic purpose in nature. (3) As we might expect, painting and carving give more direct evidence of æsthetic feeling. The fact of their existence as distinct objects shows that to some extent they have their end in themselves. Of course, many of these apparent pictures are examples of picture writing, drawn, not from delight in the forms, but in order to give information to friends. Others, again, are re-

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ligious symbols, but the majority must be classed as products of pure æsthetic feeling. Wherever the faculties of observation and execution are well developed there they are sure to find expression in an activity having its end in itself in the pure delight of creative activity. A remarkable illustration of this is given in the fact that the hunting tribes, although inferior in general culture to others, are the ones in which drawing and carving seem to have reached their greatest development, the hunter's eye and hand having been abnormally trained by virtue of his occupation. (4) The function of the dance is largely religious and social. It gives expression to the emotions of the performers and rouses those of the spectators. The fact that the primitive dance is not a performance of individuals, but of the whole tribe or village indicates its nature as an integrating agency in society, uniting the tribe among themselves and making them more effective against external foes. (5) Early poetry is undoubtedly an æsthetic phenomenon, arising as it does out of pure delight in the story or as the natural outlet of emotion. It also had an undoubted social significance and value in binding together the shares of the common literature and song. (6) Finally, music seems to be the one art of purely æsthetic origin. It seems impossible to assign to it any end beyond itself. It is the furthest removed from all considerations of practical utility. Mr. Darwin, it is true, would derive it from circumstances connected with sexual selection, but his explanation is an obvious *petitio principii*.

These conclusions may perhaps be summarized under these three heads: (1) All primitive peoples have some form of art. (2) These art forms are not always due to purely æsthetic impulses, but have a utilitarian purpose. (3) The function of early art is social preservation. With reference to our subject these results might seem to point to a negative conclusion in regard to the originality and independence of the æsthetic impulse. If art can be shown in so many cases to serve ends beyond itself, why may it not have done so universally? Why may not utility have been the mother of the arts and the essence of their meaning? And if æsthetic appreciation is thus a secondary product, reached late in the process of development, this fact is in some way interpreted to the disadvantage of art. Its naturalness is questioned, and with its naturalness, its value in itself.

On the contrary, it is necessary to note, in the first place, that art cannot be used as a means until it first exists as an end. The utility of art arises from its æsthetic quality, rather than its æsthetic quality from its utility. Unless ornament and decoration were already at

tractive to the primitive female they would have no value as an element in sexual selection. It is because they already please the eye that they play the part they do in early social life. The social function of art is dependent wholly on its æsthetic character. This holds in music, poetry, and the dance, as well as in the plastic arts. Mr. Darwin's explanation of the origin of music leaves unexplained the essential point, the cause of the agreeableness of the elementary cries. It may be contended in reply to this statement of the relations of art to utility that it takes no account of the many cases in which it has been proved that æsthetic pleasure in objects has arisen from long experience of their utility, that is, from association of non-æsthetic pleasures. This is quite true, but it is because this point involves a different problem, a problem which may be quite as important, but which is yet perfectly distinct. The one concerns the conscious nature of æsthetic feeling, the other involves the history of its unconscious conditions or origins. The one is within the sphere of art itself; the other is wholly outside these limits. The worth or dignity of art does not depend upon any theory of its origin; these pre-artistic beginnings cannot depreciate in any degree the value of the completed product. Art is distinct from its causes or antecedents. It is the same question which has been so often fought over in the history of thought, nature *versus* origin, but it is continually cropping up again in new forms demanding repeated consideration. Whatever may have been the origin of art, it exists now as an independent expression of man's nature. The only way in which its value might be questioned would be through the proof that it exists as art only by virtue of its relation to an ulterior end. Just as the ethical value of man depends upon his autonomy and his right to exist as an end in himself, so the æsthetic dignity of art consists in its sufficiency to itself. It may further social unity, must do so if it is to exist permanently, but it does so by virtue of its inherent nature. Its use as a means presupposes its value as an end, and this fact Herr Grosse's conclusions only serve to confirm. They show that art is useful, but not that utility is the essence of art.

Again, this fact may be brought out more clearly by considering the distinction between art forms and æsthetic pleasure in them, or between forms which may at one time be artistic and another time not so. The fact that certain forms once served utilitarian ends, and that the same forms at a later period gave pure æsthetic pleasure, by no means indicates identity of nature in the subjective appreciation. Identity of the object does not imply identity of feeling for it. What anthropol-

ogy can do for æsthetics is to trace the history of these objective forms, thus showing the antecedents of art, but this history of forms is not a history of the subjective feelings for art. In the truest sense consciousness has no history. Its states are eternally themselves; there is succession of these states, but they themselves remain in nature self-identical and distinct from one another. Hence it may very well be that a form which has later become known as an art form existed originally for other than æsthetic ends. It may have been useful for hunting, or clothing, or agriculture, or it may have been but an accidental variation grown dear by custom, or it may have presented a peculiarly pleasant stimulation to our perceptive powers, but it is not an object of æsthetic appreciation until ulterior ends have been lost sight of, and it is enjoyed for itself alone. The feeling for beauty is simple and not to be analyzed, whatever may have been the history of its becoming, or of the objects which arouse it. That is to say, it shares the nature of all feeling in being immediate. It is a self-evident, though apparently often forgotten, fact that all mediacy presupposes the immediate. Utility is only a secondary notion acquiring its meaning from its relation to an end. This is true both in ethics and æsthetics. The beautiful as well as the good carries us back to the nature of man as an ultimate standard beyond which explanation cannot go. The original judgment of value must, therefore, have been a simple and irreducible one, a feeling of immediate satisfaction in some action or passion congruent with the human organism. Into this instinctive judgment the question of utility cannot enter, since it in turn is founded on it as its presupposition and standard. Between this instinctive feeling and the most highly developed æsthetic appreciation there is no difference in kind; hence, unless we are prepared to deny the existence of any such immediate satisfaction, we must admit the originality of the æsthetic judgment. Herr Grosse's results, therefore, while giving us valuable information as to the conditions of primitive art, are not to be taken as furnishing any derivation of the æsthetic feeling itself, since these earliest art forms, so far as they evidence æsthetic appreciation at all, indicate that the feeling for the beautiful was as simple then as now.

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PSYCHOLOGICAL LITERATURE.

Agnosticism and Religion. JACOB GOULD SCHURMAN. New York, Charles Scribner's Sons, 1896. 16°. Pp. 181.

Two addresses (one being to the students of Cornell University) and an essay, all written in the broad rich oratorical style of which President Schurman is a master. He defends theistic religion against agnostic denials, on the one hand, and against the dogmatism of theologians, on the other. Since Kant this attitude, which is unquestionably that of wisdom, has been gaining strength; but so ardent is man's love for sharp conceptions that such vague belief as this little book expresses will, so far from being universally greeted as a happy *via media*, probably gain for its author the reprobation of influential circles on both sides. The theologians will doubtless express themselves most strongly, and in these days of wariness in official position President Schurman is to be praised for the courage with which he exposes himself to their ire. The work makes little pretence to originality of argument. The first essay is an interesting account of Huxley's career. The author yields him hearty praise, but complains, first, that he never treated religion as if it too could be a positively evolving thing; second, that he failed to see through the absurdity of the Kant-Hamilton dogma that God must be essentially unknowable to man; and third, that he too trustingly assumed that the scientific investigator as such must be the chief authority in all things, even those of the spirit. The second essay is a defence of man's knowledge of the Divinity that expresses itself in the Universe, as against what the writer calls 'the farce of nescience playing the part of omniscience in setting the bounds of science.' The last paper eloquently rejoices in the evolution of our Christian churches towards non-doctrinal theism. "If a true Christian discovers that the creed of his church is no longer tenable, his plain duty * * * is not to leave the church, but to let his light so shine that others may come to a knowledge of the fact that the church is not the mere embodiment of a creed, but the plastic organization of a life which is spiritual. His insight into the real situation of affairs forbids desertion, even though he is aware that fidelity may be rewarded by banishment or persecution" (p. 170). The little book deserves a wide success. W. J.

Studies in the Hegelian Dialectic. J. M. E. MacTAGGART.
Cambridge, University Press, 1896. Pp. xvi+259.

By these *Studies* Mr. McTaggart leaps at one bound into the foremost rank among the interpreters of Hegel, and in the course of his exegesis he displays so much ingenuity and subtlety that his book cannot but prove extremely stimulating to all who read it. It is, of course, impossible to follow him through all the depths and ramifications of his argument, but an idea of his main results may be attained by considering his answers to three of the leading questions about the Hegelian Dialectic. They are: I. What is the aim it sets before itself? II. What is its relation to experience? III. What is the significance in it of Negation?

I. Its aim, according to Mr. McTaggart, is to show that only in the Absolute Idea can the ultimate explanation of anything be found and that all other principles of explanation are necessarily inadequate. And the sole postulate it requires in order to refute scepticism and to establish all knowledge upon this impregnable rock, is the existence of experience, *i. e.*, the validity of the idea of Being, from which the Dialectic sets out. If Being is admitted, the nature of thought is such that all the other categories follow, and not even the extremest scepticism can deny that something is. But Being is the most abstract of the categories and in restoring to science the category of the Absolute Idea the Dialectic corrects the error of a course of abstraction which has been driven to equate Being with Nothing.

II. It is a mistake to suppose that the Dialectic is independent of experience or tries to reduce the universe to pure thought. When it is called a process of pure thought, that only means that it is "dependent not on experience being thus and thus, but only on experience existing at all. And the existence of experience cannot be called an empirical fact. It is the presupposition alike of all empirical knowledge and of all pure thought." And this general nature of experience is the passive basis of the dialectic movement, which is "due exclusively to that element of experience which we call pure thought" (p. 19). This indispensable but passive condition of the working of 'pure thought' forms an immediate element in knowledge (p. 41), but is not in itself knowledge. In this sense, then, Hegelism is 'without presupposition.'

III. With regard to the place of negation in the Dialectic, Mr. McTaggart holds that, so far from denying the law of Contradiction, it is essentially based on it. And, moreover, though at first and in the case of the lower categories the antithesis negates the thesis and

has to be reconciled to it by the synthesis, yet, as we pass to the higher categories, the sharpness of the opposition is gradually mitigated, until at the end we progress almost continuously. "The really fundamental aspect of Dialectic is not the tendency of the finite category to negate itself, but to complete itself" (p. 10). It follows that the Dialectic, as depicted by Hegel, does not at first fully express the nature of thought—its own nature (pp. 138-9) is in a sense subjective and represents only the way in which the human mind proceeds from error to truth. But that only brings out into clearer relief the fact that the whole truth and the sole truth is nothing less than the Absolute Idea. Mr. McTaggart somewhat hesitates to claim Hegel's approval for these inferences from his method, and admits that "the change in the type of the process is not sufficiently emphasized in Hegel," but he regards it as necessary, "since it is only by the aid of some such theory that we can regard the system as valid at all" (p. 158).

After this comes a chapter on that sorest of vulnerable points, the relation of the Dialectic to Time, concerning which I have had my say elsewhere,¹ and two chapters on the final result and application of the Dialectic. In these latter McTaggart drops the rôle of reverent discipleship and in his own name reaffirms objections against which he had elaborately defended Hegelism in the earlier chapters, denying, *e. g.*, that pure thought and the philosophy which systematizes it is an adequate expression of the whole nature of the Spirit, and that the applications are the really valid part of Hegel's system (p. 238). Some of his conclusions here seem strange emotional exotics to grow upon the arid and alien soil of Hegelism, *e. g.*, that all reality consists of *spirits* which are individual (p. 222). But after all the main questions suggested by his book are: (1) Will his interpretation of Hegel stand? And (2) if it will, what does Hegelism amount to?

Of the first of these questions it would be unbecoming to essay a decision while life-long students of Hegel show the reticence and caution observable in Professor Wallace's review in *Mind* (N. S. No. 20). And after all science is more concerned with the validity of Hegel's plea as presented by Mr. McTaggart than with the actual meaning of a writer who certainly neglected many opportunities for speaking out clearly. Hence the second is the question of more pressing importance, and an answer will probably be most facilitated by a critical discussion of the three characteristics of Mr. McTaggart's interpretation stated above.

¹ *Mind*, N. S. No. 13. *The Metaphysics of the Time-process.*

I. It may be pointed out, to begin with, that only a very accommodating sceptic would assert Being in such a sense that the whole Dialectic can be extracted from it. The ordinary kind would probably object that Mr. McTaggart's argument most palpably involved the characteristic Hegelian confusion of essence and existence, and that the admission of a (possibly illusory) appearance of existence did not carry with it the validity of the *idea* of Being.

As to II., it is very hard to construe the independence of experience which Mr. McTaggart ascribes to 'pure thought.' The Dialectic is a process of 'pure thought' which is represented as the active principle in knowing, whereof it monopolizes the credit. Yet it is admitted to be abstract (*e. g.*, p. 18, 105, 233), *i. e.*, the product, together with pure sensation, of a merely logical analysis of the actual process of knowledge which alone is a concrete experience. We are expressly warned (p. 74) that "the importance lies only in the concrete whole," and that "this reality is not to be considered as if it were built up out of thought and sensation." It follows that "pure thought" "never really exists except as an element in experience" (p. 105), *i. e.*, it is never found as a fact at all. How then can the Dialectic be a description of any actual process of knowledge?

Further, it is doubtless true that the 'lower' categories are abstract and very far from the concreteness of the actual. But is this any less true of the highest category, of the Absolute Idea itself? Mr. McTaggart talks as if it were concrete, but it is concrete only in the sense of coming at the end of an unavailing effort to transcend the abstractness of all thought. To become really concrete, the Dialectic would have to get back to the concrete individuality from which abstraction started. Why, if it has such a horror of abstraction, did it ever abandon it? That is a vital question for all such schemes of thought. For they are all rendered superfluous by the recognition that knowledge serves a purpose, that it is always necessarily abstract, that the abstraction is useful, and progressive because it is useful. In the whole process it is only the first step which costs, the step that takes us from the concrete individual to the abstract universal. But after that everything is plain sailing, requiring no justification; we proceed gaily to the highest abstractions, nay to the idea of Being—a symbol so abstract that its content cannot be distinguished from nothing—whenever such abstraction is needed for our calculations. Such is the state of things which Hegelism so elaborately misunderstands that it feels bound to prove, by an (unsuccessful) reduction to their starting point, the validity of instruments of thought which are

fully sanctified by their usefulness. And all for what? To justify, it is said, the use of 'higher' categories. But is it not simpler to defend their validity by recalling that the lower originally proceeded out of them by progressive abstraction? The Dialectic undoes the abstraction of science—but had science no reasons for its abstractions, and if it had, will it not suffice to remind it of those reasons? What need then for the Dialectic?

III. The same question is echoed by Mr. McTaggart's conclusions as to the subjective element in the Dialectic. If "the opposition of one idea to another and the consequence negation and contradiction do not mark any real step towards attaining the knowledge of the essential nature of thought" (p. 147), if the Absolute Idea alone is adequate, then it is surely better never to lose sight of it than to recover it by a dialectic process which, in spite of Mr. McTaggart's utmost elucidations, remains an enchanted forest in which the babes in philosophy are sure to lose their way. To admit that not the Dialectic itself, but only its result, can pretend to absolute truth, is surely to reduce it to a pedagogical method due to the infirmity of human intelligence. And not only is the method bad pedagogically, but no cause is shown why it should be the only method. If the Absolute Idea (or better still, as shown above, the concrete individual) is to be reached, the shorter and simpler the method the better. And better methods readily suggest themselves. The necessity of ultimately recognizing the anthropomorphic basis of our interpretation of our experience—for that according to Mr. McTaggart is what the Dialectic demonstrates—may easily be made clear both directly and indirectly. Directly, by showing that none of the categories used in science or ordinary life ever free themselves from their human reference; indirectly, by showing that the lower categories annul themselves when taken as independent. Both these methods would seem far preferable to the illusory starting point, the paradoxical phrasing, the cumbrous and obscure progression of the Dialectic, which seems nothing but a highly contentious way of reaching assumptions which in science and ordinary life we accept without contention and in philosophy can justify far more simply. So that to me at least it seems not the slightest merit of Mr. McTaggart's work to have given fresh urgency to the question: What, then, is the good of the Hegelian Dialectic?

F. C. S. SCHILLER.

CORNELL UNIVERSITY.

S. Kierkegaard als Philosoph. HAROLD HÖFFDING. Stuttgart, Frommann, 1896. Pp. x+170.

This brochure is the third in *Frommann's Classiker der Philosophie*, a series similar to *Blackwood's Philosophical Classics* which is being issued under the editorial supervision of Prof. Falckenberg of Erlangen, already well known through his *Grundriss der Geschichte der neueren Philosophie*. Besides the present work, volumes on *Fechner* by Professor Lasswitz and *Hobbes*, by Professor Tönnies have already appeared. Among the notable announcements of numbers to come are Riehl's *Hume*, Paulsen's *Kant*, Höffding's *Rousseau*, Lasson's *Hegel* and the volume on *Lotze* by the editor. The series will be of especial value because of its additions to our list of standard compendiums on the classical writers and systems.

Kierkegaard (1813-1855) finds a place in the series as the foremost thinker which Denmark has produced (p. 2) and as a notable personality in the philosophico-religious movements of the century. Professor Höffding leads up to his subject proper by chapters on *Die romantisch-spekulative Religionsphilosophie* (Schleiermacher and Hegel), *Kierkegaard's ältere Zeitgenossen in Dänemark* and *Kierkegaard's Persönlichkeit*. Then follows the discussion of Kierkegaard's philosophy under the two principal heads of epistemology and ethics. This forms the body of the work, which concludes with a somewhat briefer explanation and criticism of the philosopher's attitude toward the Christian faith and his breach with the 'weakened and softened Christianity' of the Church. Central in the whole development and of great psychological interest is the influence of Kierkegaard's temperament upon his speculation. Possessed by an inherited melancholy tendency, extremely conscientious, and with a dialectical gift which forbade him to glide over antinomies, he reproduced in his thinking, especially in his ethical and religious conclusions, the lonely individualism, the unceasing inner conflict, the paradoxical outcome of his life. In the beginning he is satisfied neither with Hegel's speculative theology and its impossible iteration of the threefold rhythm nor Schleiermacher's easy renunciation of a direct knowledge of the absolute. As he frames his own ethic, he emphasizes freedom and the essential individualism of moral culture, only to void morals of all social content and, by giving them an exclusively transcendent basis, to reduce morality to asceticism. Toward the close of his life he feels himself compelled publicly to censure the existing Christianity as a degenerate travesty of the pure religion of Christ and to demand a return to the unworldly simplicity of the

primitive Christian community. Then, worn out by his labors and his sufferings, he dies when only forty-two, after profoundly affecting the thought of his time and country.

The book is written with the customary skill of its author. The touch is so deft that the reader wishes it were possible to read the Danish original of Professor Höffding instead of the German translation; and so sympathetic, in spite of grave differences of position between the subject and the writer of the work, that he is ready to agree with the opinion expressed in the preface by Schrempf, one of Kierkegaard's principal German admirers: *Dass hier ein Philosoph der Continuität den Irrationalismus Kierkegaard's darstellt und auf seinen wirklichen Wahrheitsgehalt prüft, kann auch der Lehrer Kierkegaard's nicht bedauern, sondern nur mit Freude begrüßen.*

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Ueber physische und psychische Kausalität und das Prinzip des psycho-physischen Parallelismus. MAX WENTSCHER. Leipzig, Barth, 1896. Pp. x+122.

The two essential elements of modern psycho-physical parallelism are stated by the author to be: first, the assumption of a uniform parallelism between any given psychical process and its corresponding cerebral process; and second, the affirmation of an entire absence of any causal interaction between these two kinds of processes. Wundt lays greater emphasis on the second part of the doctrine, but concedes the impossibility of consistently maintaining an absolute independence of the individual consciousness. This principle, which is advanced by its supporters merely as an expression of empirical facts, is in reality a metaphysical doctrine, for it goes beyond experience in all its teachings, and if it were really based on observed facts it could serve only as a preliminary formula and would require some explanation.

A study of physical causation shows the impossibility of proving that we are here dealing with a closed, independent system of processes. We can discover only the phenomenal aspect of causation; its essential nature is beyond the reach of our observation. That any description of phenomena by the physical sciences should seem to support the doctrine of an independent physical causation follows from the circumstance that only physical facts enter into the discussion. The extension of the principle to realms in which other kinds of facts enter in, is not justified by its apparent confirma-

tion in the physical sciences. But in the natural sciences themselves, even if we admit the assumption that the amounts of energy in the cause and in the effect are equivalent, we are by no means forced to admit that no outside agent can enter into the process. Take, for example, a case of potential energy which is converted into kinetic energy. The moment at which this stored up energy shall begin to discharge is not determined by the energy itself. It is determined by circumstances which do not depend on the expenditure of any physical energy. If some liberating cause sets the process in operation the energy of this liberating cause is not destroyed in the act of bringing about the discharge, but its energy is added to that of the efficient causes, and its equivalent appears in the effect. The determination of the moment at which a cause shall operate may thus be effected by some agent without the expenditure of any physical energy. In this way we have a reasonable explanation of the frequently observed fact that the psychical processes determine the moment at which certain physical processes shall take place, without there being any demand for additional physical energy either in the cause or in the effect.

Living organisms exhibit individual peculiarities. We have here in the physical world certain groups of processes obeying laws which are peculiar to themselves. Such organisms may well be regarded as mediators between pure physical processes and processes which are non-physical in their nature.

Psychical causation is limited to certain individuals of a unitary character; their unity consists, not of some objective relation of parts, but of immediately perceived unity in consciousness. Such individuals are capable of communicating with each other only through the physical world. The question arises, are these circles of individual, unitary consciousness entirely closed to the action of any external cause? In their origin they can not be regarded as independent. Breaks in the temporal continuity of the series of processes and the appearance of new processes, such as sensations, can not be explained from the foregoing conscious states or conditions, but require the action of some outside agency. Psychical activities are not determined by the temporal relations of outside causes, but by the relations which exist between the actual contents of processes resulting from these causes. It is the logical, ethical or æsthetic relation between contents of consciousness that leads to volition. The subject thus determined by relations of content may, in the manner indicated, influence the temporal order of physical processes.

Instead of parallelism between two independent series of processes

we have, then, a form of interaction which may be called causal. The attempts to avoid the word cause by using the term occasion are mere verbal evasions. This kind of an explanation, formulated in the spirit of Lotzean philosophy, seems to the author to meet the observed facts and metaphysical requirements involved, better than any form of parallelism.

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Grundriss der Geschichte der Philosophie. JOHANNES REHMKE. Berlin, Duncker; New York, G. E. Stechert, 1896. Pp. 304.

This is a compendious volume covering the whole range of European philosophy from Thales to Lotze. It differs from many of the recent Outlines in its almost entire exclusion of bibliographic material, titles of works of the authors discussed as well as specific references to these works being rarely introduced, and no reference being made to other histories or to monographs covering the same ground. It is not a 'manual,' therefore, in the sense of being a guide to study beyond itself; its value lies in its own individual interpretation of the systems of which it treats. It differs also markedly from some of the smaller and many of the more extended Outlines in that it does not attempt at any point to give the general historical setting of the philosophical movement, but confines itself rigidly to an account of the substance and relations of systems of philosophy proper—*wissenschaftliche Philosophie*. So complete is this abstraction that even the specific contributions of Christianity and the influence of the modern scientific movement alike receive no recognition. It may be questioned whether such a method of treatment can be in the fullest sense true; and, from the pedagogical point of view, one may doubt whether it conduces to the best philosophical culture. At the same time, it is but fair to judge a book by what it *does* give us within the limits it has set for itself, rather than by what it purposely does not give. And we find, on examination, an unusually clear, vigorous and interesting presentation of the leading systems and schools from ancient to modern times. The author's interest is evidently strongest in the direction of Metaphysics and *Erkenntnistheorie*. He has given much more than a bare statement of principles and doctrines; rather we find a sympathetic and thoughtful interpretation, and occasionally, when the author gives himself room, a fine logical and psychological analysis and development of the problem in hand. This is notably the case in his account of Kant. The treatment is 'objec

tive' in the best sense, with frequent reference to the relations, positive and negative, which one system bears to its predecessors. Only now and then does the author let fall a criticism or suggestion which indicates his own point of view, *e. g.*, pp. 24, 250, 295.

Dr. Rehmke divides his work into Ancient and Modern philosophy. Notable here is his inclusion under the former head, not only of the philosophies of the Hellenistic period, but also of Scholasticism and the philosophy of the Renaissance, on the ground that these are essentially only pupils of the Greeks. The Christian Middle Ages is treated very briefly, only 15 pages. Modern philosophy is divided into Pre-Kantian, Kantian and Post-Kantian. Naturally a large place, 46 out of 200 pages, is given to the exposition of Kant, while Post-Kantian philosophy gets a space of only 44 pages. The chapter on Kant shows the author at his best, and is decidedly a fine piece of work. Naturally a considerable preponderance is given by the author to Continental and especially German thought. The more recent English thought is omitted entirely.

A few minor errors have caught the eye in a somewhat rapid reading: on page 4 (l. 26) Anaximander stands instead of Anaximenes; on page 84 the date of Philo is given wrongly; on page 103 Bacon's famous simile for final causes has strayed from its original connection.

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Die Impersonalien. M. JOVANOVIĆ. Belgrade, 1896. Pp. 143.

After a short introduction in which he deals with the history of previous investigations, Jovanovich presents his point of view and then enters upon the consideration of his subject proper, the impersonal judgment, under the three heads—origin, function, limits.

Previous investigators mistakenly isolated the grammatical and the psychological-logical points of view. On the one side an identity of thought and language was maintained; on the other there was a discrepancy. The representatives of both views fell into hopeless confusion and contradiction. A true estimate does not admit the identity of thought and language nor a discrepancy between them.

Anthropology forbids us to maintain that impersonal judgments are the original embryonic forms out of which all others have been differentiated. Animism as expressive of the earliest form of experience teaches us that primitive man interpreted all outer occurrences in terms of his own personal life; the clouds and heavens

'rained.' In the mythological age these personal activities were generalized; Jupiter, Zeus, Indra, 'rained.' Finally, when thought freed itself from personification, the causes of certain experiences became completely undetermined and unknown. At this stage the impersonal 'It' arose.

The function of impersonals was and is that of indicating a subject which is altogether unknown, but which, nevertheless, the mind must still think.

From this also the limits of the impersonal are clear. All endeavor to determine the subject, in whatever degree this determination may be presented, is artificial and arbitrary. It inevitably leads to confusion and perversion of meaning. Our only method of classification must be based solely upon the different kinds of experience which are referred to the unknown subject.

So much for Jovanovich's own treatment; the monograph is, throughout, dependent entirely upon Wundt's interpretation. In fact, the author's deference to his master, and his confident assertion of the falsity of views differing from his own, do not seem to be consistent with the supposed impartiality and scientific thoroughness of the German student.

The investigation is indicative of the difficulty which has met all enquirers in this field from the time of the Greek grammarians. The impersonal judgment has been considered an anomaly which must be dealt with from the standpoint of certain presuppositions. Underlying Jovanovich's treatment I find these: (1) All experience is objective; (2) the fundamental relation is that of subject and object; (3) judgment consists in the uniting of thought and reality, *i. e.*, it is discursive.

The mere statement of the first two presuppositions will suggest the criticisms likely to be made. The third is the most important. If we admit that all judgment is discursive then a subject must be sought for the impersonal. Predication without a subject of predication is a contradiction. But, on the other hand, no subject has been found for the impersonal. Is the controversy then to be continued forever? The difficulty might suggest that it would be profitable to lay aside our presuppositions, in order that we might understand the impersonal not as a judgment, nor as having a subject or a predicate, but as an experience. This point of view leads, in my own opinion, to the following result. The impersonal presents us with a situation immediately recognized as such. It distinguishes itself from intuition in that the impersonal is vague, schematic, while the intuition is clear and

definite. Again, certain forms of the impersonal indicate a breaking-up of this immediate recognition and a reference to a vague whole not immediately grasped. Now in as much as reality is grasped, differentiated or measured in the impersonal, we may rightly call it a judgment. But it is a judgment in which subject and predicate do not appear. This analysis leads us to believe that the essential nature of judgment is recognition or differentiation, not reference. In the impersonal and intuitive forms recognition is immediate; in the ordinary discursive form it is mediate.

This view is supported by child psychology in which we find definite situations or realities recognized before there is any use of noun and verb. Again, comparative philology shows that the noun cannot be derived from the verb and *vice versa*, but it points (as Jovanovich admits) to a stage in thought where they were simply implicit. Finally, when we recognize that in the child's consciousness the use of noun and verb, and the recognition of a self as opposed to an object, arise together, we see how our theory fits in with the necessity felt by Romanes of getting a connecting link between the perceptive processes of animals and the conceptual processes of man. The impersonal recognizes the facts which Romanes brought forward and frees his position from the logical entanglements which it presents, in giving us percepts apart from concepts.

Thus as the immediate recognition (though in a vague schematic way) of reality and the beginning of a reference to a mediately recognized whole, the view of the impersonal above presented unites the various conflicting theories. As immediate recognition there is neither subject nor predicate; as a vague reference to a larger whole a subject is found in varying degrees of determinateness. Finally, when the "It" represents merely a shorthand way of indicating a familiar object, we have the singular judgment.

S. F. MACLENNAN.

UNIVERSITY OF CHICAGO.

Quelques Remarques sur L'irréversibilité des Phénomènes psychologiques. E. HALÉVY. Rev. de Met et. de Mor., Nov., 1896.

M. Halévy's article is, in the main, a criticism of the attempt made by psychologists of the Association School to apply to mental phenomena the principle of mechanical reversibility. In a purely quantitative science like geometry all terms have the same *logical* value, and may be defined in the same manner with *change of sign*. If all Force be reduced to modes of motion the same doctrine will hold in

the case of the physical sciences. But does it hold when we come to deal with mental phenomena? Is the past interchangeable with the future, and can memory be substituted for will? Psychology cannot be classed among the purely positive sciences until this seeming irreversibility of its phenomena is explained. Hence the attempt made by M. Ribot and others. These theories M. Halévy examines.

The one postulate of Associationism is 'a succession of states following one another according to the laws of resemblance and of contiguity.' A series of states related to one another by these laws would be completely reversible. If 'past' and 'future' as psychologic states follow this same order, one can be substituted for the other. Memory and will also become psychologic functions, one the inverse of the other. Associationism has to explain their apparent difference.

Suppose we say that the difference is that between a *present* state and the associated state *not present*, that does not tell us why the not-present is named sometimes future, sometimes past. We do not dispose of the difference by proving that the psychological *process* is the same (association) when a state is referred now forward, now backward. Again, M. Ribot seems to give up the problem when he assumes that the present state has duration—hence a beginning and an end, giving rise in present consciousness to an immediate intuition of past and future.

But suppose the distinction is, in part at least, a function of will. The past is that which is determined, the future is dependent in part upon my undetermined volition. If this be true it remains for Associationism to explain their apparent difference—they must become reversible; likewise the distinction between 'me' as cause and 'thing' as cause. Volition differs also from foresight (prevision). If this difference be abolished, freedom of the will becomes identical with foreknowledge. "To be free is to *know* what one will do and why one will do it." This complete convertibility of phenomena demanded of Associationism, M. Halévy concludes, is impossible.

In the second part of the paper he takes the ground that while 'past' and 'future' are irreversible in the sense of being convertible, yet that both may be reduced to terms of logical succession. There is a distinction to be made between the order of desire (*vouloir*) and the order of perception (*percevoir*). In the first case we proceed from *end* to means—D C B A. In the other we proceed directly from means to end—A B C D——. There is a sense in which the series A B C D is completely irreversible, and of this Associationism

fails to take account, viz, in the order of *logical* representation the conclusion cannot precede the premise. But this series may be interpreted in time, either in the order of will or in the order of perception, one the inverse of the other. Thus there is no contradiction between psychic reversibility and logical irreversibility. Synthesis, for example, represents an interpretation in the order of perception, which is the same in direction as the logical order. Analysis, on the other hand, corresponds to the order of volition.

M. Halévy rejects the associationist's identification of past and future, and rightly. But can they be reduced, as he thinks, to terms of logical succession? In the first place, the logical order does not involve time, and is interpreted as *succession*, only when the idea of time has arisen from some other source. Again, the order of perception of phenomena is not always logical, yet they get referred to their appropriate places in the time-series. So the arrangement of events in the future by imagination does not follow always their logical order. Genetically the child remembers before it reasons; the time order does not depend on logical sequence. We are inclined to think that the reference of things forward and backward is as fundamental as perception itself.

PRINCETON UNIVERSITY.

J. M. TROUT.

EXPERIMENTAL.

Observations sur quelque Types de Reaction simple. TH. FLOURNOY. Geneva, Librairie Ch. Eggiman & Cie., 1896. Pp. 42.

The object of the series of investigations of which the present monograph is a report is to determine how many clearly-marked types of simple reaction there are and what are the characteristics of each. By a type of reaction Professor Flournoy means the way in which a comparatively unpracticed subject reacts in the shortest time. In the course of the experiments he tested about seventy students of both sexes. The number of reactions obtained from each subject is very unequal, but unity of method makes the results comparable. The reactions were all taken with the d'Arsonval Chronometer and are, therefore, not reliable for absolute time. Their value lies in the comparison of group averages. The tests were made in series consisting of two, and sometimes more, groups of about fifteen reactions each, in which the direction of attention was the only factor varied from group to group. To avoid the possible effect of fatigue, the temporal order of the series was varied from group to group. The

stimuli used were tactual, visual and auditory. The response was the closing of a key by the index finger. Careful notes were kept upon the bodily attitude and the subjective experiences of the reactor during each group of reactions, upon whose importance Professor Flournoy lays great stress. From the results thus obtained, Professor Flournoy differentiates four principal types of reaction, two of which are divided into sub-types. They are: 1, the motor type, consisting of (a) the natural motor type and (b) the forced motor type; 2, the central type; 3, the indifferent type, and 4, the sensory type; subdivided into (a) the visual motor type and (b) the kinesi-motor type. The criterion for the separation of the principal types is the shortness of reaction time; that for the separation of the sub-types, the subjective condition of the reactor. Each type is named from the direction which the attention takes when the reaction time is shortest. In all cases, except the forced motor type, the shortest reaction was also the one naturally adopted, the easiest and the most automatic. Professor Flournoy does not consider his enumeration of types exhaustive. It includes only those which have come out clearly in his experiments. Mixed and indeterminate cases of his own, as well as the logical possibilities of the case, make him think that further research may differentiate more types. Beside the data mentioned, Professor Flournoy has kept a record of the nationality of the reactor, the branch of work he was pursuing and his type of mental imagery. It is to be regretted that detailed results from only seven of his seventy subjects are published and that we are not told what proportion of them belong to each type.

Although Professor Flournoy expressly states that his work is not intended as an interference in the long and heated discussion about the existence of simple reaction types other than the type of Lange, and the relation of reaction type to mental imagery, his investigations deal directly with both these questions and cannot but have a bearing on them. In opposition to the Leipzig investigators, he finds that there are types (in his sense of the word) of simple reaction other than that investigated by Lange. Whether or not these types could be reduced to the Lange type by indefinitely long practice, he leaves an open question. With the amount of practice he gave his subjects, he finds no indication that the other types of reaction approach the motor. On the contrary, practice, as far as he has observed it, tends to reduce the time of the typical reaction faster than it does the times of the other reactions, with the result that the characteristic difference in time is lengthened and the types become more distinctly marked. In so far as Professor Flournoy's results corroborate the

type theory of reaction, they agree with those obtained in the laboratories of Princeton and Chicago. But his observations on the effects of practice are just the opposite of the Princeton and Chicago results. Professor Baldwin, who is motor in type, noticed incidentally that his sensory time approached his motor time with practice. Professor Angell and Mr. Moore, whose primary interest was in the effect of practice on reaction time, found in all three cases which they investigated that the non-typical reaction approached the typical with practice. According to the theory of reaction type advanced by Professor Angell and Mr. Moore the effect of practice would necessarily be to decrease the difference in type. The facts at command are insufficient for any dogmatic statement. Professor Flournoy gives statistics on the effects of practice for only one case. The other four cases for which we have statistics on this point (Professor Baldwin and the three subjects tested at Chicago) all give results opposed to Professor Flournoy's. In the tests made at Chicago it was found that it took some practice for the two types to emerge clearly, but that after they were once clearly differentiated they began to approach. In as much as in Professor Flournoy's isolated case the two reaction times were at first practically identical (sensory 137, 5 σ , m. v. 22; motor 140, 2 σ , m. v. 28, 3) and the reactor found great subjective difficulty in getting a genuine motor reaction at all, it is at least possible that it took a much greater amount of practice than usual for the two types to become differentiated; that his results stop with the differentiation of the type, and that further practice would have made them approach one another. The number of reactions, however, is as great as the number which, in the tests made at Chicago, sufficed for both differentiation and approach of type.

From the statistics which Professor Flournoy kept with regard to the type of mental imagery of his reactors he agrees with Professor Baldwin that the general tendency of the individual to use sensory or motor images corresponds with his reaction type. But any assertion that reaction type corresponds to mental imagery so closely that one can be determined by the other he thinks is more than the facts at present warrant. The mixture of type and fluctuations of imagery found in a single individual, and the large variety of possible sensory-motor coördinations, make it seem improbable that any hard and fast relationship between the two can ever be formulated. Professor Flournoy makes the suggestion that nationality may prove to be as much of an index of reaction type as mental imagery.

HELEN B. THOMPSON.

UNIVERSITY OF CHICAGO.

The Effects of Odours, Irritant Vapours and Mental Work upon the Blood Flow. T. E. SHIELDS. The Journal of Experimental Medicine. Vol. I., No. I. 1896.

The author, in summarizing his results, tells us that "the most important outcome of this investigation has been the completion of various improvements in the construction and use of the plethysmograph, by means of which numerous errors attending the use of the instrument have been eliminated." It is, in fact, a far way from the apparatus employed by Fick, Mosso and Lehmann to the elaborate contrivances described in this dissertation; and in view of the many problems whose solution has been sought in this line of work, the more important of these modifications deserve notice.

With earlier forms of the plethysmograph it was doubtful whether the changes recorded were due to variations in the volume of blood or to movements of the enclosed arm and fingers. This source of error Shields eliminates by means of an arm-holder which, without hindering the circulation, keeps the arm rigidly in place and prevents panting of the elastic sleeve. Again, in the records hitherto obtained, the pulse and the gross volume changes were shown in the same curve and tended to mask each other. To separate these, the vasomotor effects are registered by a suspended test tube (Bowditch), while the pulse effects are taken care of by an air cushion which responds to smaller waves from the arm cylinder and transmits them to the Marey tambour. The volume and pulse changes, along with the pneumographic and time curves, are inscribed by frictionless glass pens upon a horizontal kymograph so constructed that a continuous record of any desirable length may be obtained. For psychological purposes the main advantage of this arrangement is that a whole series of reactions may be studied in their mutual connection, and without the disturbance occasioned by change of kymograph drums.

In the first class of experiments of which an account is given, various odors were administered to the same subject through tubes ending in an odor plate, and were controlled by electric valves in such a way that nothing could be known of the stimulus except through the sense of smell. In a second series of experiments, twelve subjects were tried. In addition to the effects produced by odors and vapors, other changes were noted and attributed to 'mental activity,' but the precise character of the stimulation which called these forth is not sufficiently indicated.

The results, illustrated by plotted curves at the close of the dissertation, show that olfactory sensations, irritant vapors and mental

work cause a diminution in the volume of the arm. "Whenever the stimulation (odor) occasions an increase in the volume of the arm, as sometimes happens, it seems to be due to acceleration of the heart rate, which, of course, tends also to increase supply of blood to the brain." But no support is afforded to the view "that pleasant sensations are accompanied by a diminution of the blood supply to the brain and unpleasant sensations by the reverse effect." In the statement of these conclusions and throughout the dissertation, there is a cautious tone which in no way lessens the value of the work.

E. A. PACE.

CATHOLIC UNIVERSITY, WASHINGTON.

Attention: Experimental and Critical. By FRANK DREW.
American Journal of Psychology, VII., 533-573. 1896.

The experimental part of Dr. Drew's study consists of three distinct lines of work: *A*, measurements of reaction and association times of various degrees of complexity under various conditions of distraction; *B*, a qualitative study of association by Galton's method (*Human Faculty*, pp. 185 ff.) with concentrated and distracted attention; and *C*, a study of the recognition of the order of nearly simultaneous stimuli with voluntarily directed attention.

Though many reactions were taken for *A* and the general results were in substantial agreement with those of other observers, they were not regarded as satisfactory and no use is made of them here except as they furnished introspective and other casual observations.

In *B* the question was: What effect, if any, is produced in the normal run of association by distraction? Tests were made in parallel series: in one the experimenter looked at the stimulus word and then gave himself up to securing as many associations as possible within a fixed interval, at the end of which those gotten were noted; in the other he tried to do the same thing while adding a number of digits requiring an approximately equal time. Four sets of 100 stimulus words each were used and each set was gone over twice at intervals of a month, the repetitions being sometimes arranged to duplicate the first conditions and sometimes to alter them, as shown in the following little table.

First time.	Second time.
1st Set: Distraction	Distraction
2nd Set: Concentration	Concentration
3rd Set: Distraction	Concentration
4th Set: Concentration	Distraction

In this way over 3,000 associations were collected and treated statistically. The most striking result is the relatively small effect produced by the adding.¹ There is a somewhat larger proportion of fresh associations in the second trial of sets first gone over with distracted attention than in those first gone over with concentrated attention; the sets with concentrated attention show more associations from the last three-fifths of life (excluding the immediate past); there is an indication that word jingles and purely verbal associations are interfered with by the adding, probably because the language apparatus is partially taken up by that activity; but in almost every case the percentage of difference is small. This result, though at first surprising, is not so strange when the conditions of the experiment are regarded. It seems likely, on the one hand, that the haste of getting the greatest number of associations in a limited time and from a single word is a distraction in itself, and on the other that many associations in the series with adding are secured in momentary wavering from that task. Two incidental observations are of some interest, namely, that the first thing to follow the sight of the stimulus word (when the novelty of the experiment had worn off) was almost always a mental pronunciation of it which furnished the nucleus from which the associations developed; and second, that almost all associations were given a 'spatial setting' or localization in motor or visual-motor terms. This latter was often the first thing to come and was tardily followed by the other elements of the association.

The third line of experiments had to do with the time order of nearly simultaneous stimuli, and the question took this form, namely: Given a pair of stimuli (two clicks, for example, addressed one to each ear) so near together that their order can just be recognized, will any change be produced by voluntarily attending to one or the other? A very little work on this matter had already been done by Dr. Alice J. Hamlin (*American Journal of Psychology*, VI.), but with negative results. The stimuli used by Dr. Drew were telephone clicks (one to each ear), electric shocks (one to each hand) and a click and shock to ear and hand; the interval for the first two pairs was 0.024 sec. and for the click and shock 0.031 sec. Parallel series with balanced attention and attention concentrated on one side or the other were taken in considerable variety, chiefly upon two subjects. The

¹This experiment was made by Drew on himself. A similar series under slightly varied conditions was undertaken by the writer at the same time and the statistics partially worked up. Drew's general negative result is supported by them.

following are the most important results: With two clicks concentration of attention is a positive hindrance, fewer right judgments being made on the average, and still fewer (relatively) when the stimulus arrived first on the side to which attention was directed, due possibly to an unconscious change in the manner of judging. To test whether the criterion by which the order was judged was one of intensity, experiments were made with loud and faint clicks, and it was found that both subjects tended to call the fainter the earlier. With shocks it was found (contrary to the click results) that attention to one side or the other favored the stimulus received on that side. With strong and weak shocks the fainter again seemed earlier, but the tests on this point are few and not fully accordant. When the subject was distracted from both shocks by being required to read aloud, there was in no case a decrease in accuracy and sometimes a decided gain. The experiments with disparate senses (ear and hand) show, with balanced attention, a strong bias in favor of the order shock-click, which renders difficult the interpretation of the results with directed attention. Both subjects show gain with directed attention, but disagree as to whether it is more advantageous when directed toward the leading stimulus or the other. Such biases were also found at other stages of the work and add greatly to the laboriousness of the experiments and the complexity of the results, especially as they were not constant, but underwent slow changes as the research continued. Introspection under the conditions of the experiment was also singularly difficult and threw little light upon its real nature.

To make a generalization which shall unify these varying results is hardly possible until further experiments have established more fully the effects of several of the factors involved. While not undertaking to do this, Dr. Drew makes many suggestions and presents a theory of perception, apperception and attention which was developed in considerable part by these experiments. It is an extreme example of those that would reduce mind almost or quite to kinæsthetic terms. For the detail of it the reader must consult the paper itself, though with the warning that in parts the thought suffers much from obscurity of presentation.

CLARK UNIVERSITY.

E. C. SANFORD.

ANTHROPOLOGY AND CRIMINOLOGY.

Psychologie der Naturvölker. Ethnographische Parallelen.

JACOB ROBINSOHN. Leipzig, Friedrich, 1896. Pp. i + 176.

This is a very comprehensive title for a rather contracted work.

The author has made a creditable compilation of records regarding the primitive conception of the soul, but his work was anticipated twenty-five years ago by Tylor and in a much more thorough and philosophical spirit than that exhibited in the book before us. There is hardly a single point brought out by Herr Robinsohn that is not treated in Tylor's chapters on animism in his well-known 'Primitive Culture' and, while the amplification of evidence is welcome, there is not much more to be said for it.

There is, on the other hand, a good deal to be said against the waste of energy on an already well-threshed subject while so many fruitful ethnological fields are left untouched. This is not an argument against rehandling an old topic, especially in a new light, but our author has neither new light nor new methods, yet his book appears with all the air, though not the express claim, of an original and valuable contribution to ethnological knowledge. Further, as already implied, the title is misleading. One opens the book expecting naturally a treatise, good or otherwise, on comparative psychology, but it is a sad day for our science if the psychology of primitive man is to be confined to primitive man's conception of his own non-bodily self, which is what we are offered here.

By way of praise, it may be said that the subject-matter is well arranged and well handled, the bibliography is useful, and the whole would be a capital piece of work if it had not all been done before. As it is, it is a very good guide-book to Tylor.

LIVINGSTON FARRAND.

COLUMBIA UNIVERSITY.

The Female Offender. C. LOMBROSO and W. FERRERO. With an Introduction by W. DOUGLAS MORRISON. New York, Appleton, 1895.

In the present volume, one of the 'Criminology Series,' edited by Douglas Morrison, Lombroso's '*La Donna Delinquente*' has been made accessible to the English speaking public. The work is a typical example of Lombroso's acute observation, but also of the entire inadequacy of his statistical methods. No variation from the normal type escapes his notice, but the comparison of the frequencies of their occurrence among various social groups is entirely unsatisfactory, because it is based on very small series of observations. To give an instance: Plagiocephalism is found in 42% of 66 male criminals; in 17.2% of normal women; in 28.8% of 55 criminal women. Therefore, the error of mean squares of the first group is about 6.3%, that

of the last group 6.7%. The difference between these two groups is 13.2%, but its error amounts to 9.1%. Therefore this difference of frequencies of occurrence is very uncertain. The theory of probabilities proves that we might expect these results to be fundamentally changed if the number of observations were increased. In the instance given here the differences of observed values are great; in most cases they are entirely inside the limits of errors. We hold, therefore, that Lombroso has not succeeded in establishing the validity of any of the characteristics of the criminal type which he claims to have discovered.

It is true that signs of degeneracy are frequent among criminals and that a correlation of degeneracy and of criminality exists which may be directly physical, but which is likely to be, to a great extent, social. A consideration of the fact that these signs are not confined to the criminal classes shows that it is an incomplete correlation with which we have to deal. That is to say, signs of degeneracy are sometimes, not always, connected with criminality and, *vice versa*, criminality is sometimes, not always, connected with degeneracy. Therefore the problem, in order to be satisfactorily solved, should be treated in a manner differing from that applied by Lombroso. Setting aside the necessity of basing descriptions on much more extensive series which would enable us to prove that differences are not merely accidental, both aspects of the correlation should be investigated. We must not only gain systematic knowledge of the characteristics of the criminal classes, but also of the general distribution of each of these characteristics among a variety of classes not criminal. Only in this manner can we hope to understand their significance.

While Lombroso will always deserve the credit of having forcibly called attention to the physical and psychical characteristics of abnormal man, his statistical methods are so unsatisfactory that very few of his results can claim to be well established, and I believe hardly a single one in the volume under review can be considered as proved by the material offered.

COLUMBIA UNIVERSITY.

FRANZ BOAS.

CHILD PSYCHOLOGY.

Paidologie. Entwurf zu einer Wissenschaft des Kindes. OSCAR CHRISMAN, In Diss., Jena, 1896. Pp. 72+24 pages of bibliography.

This is a sketch of 'Paidology' by the original inventor of the term. The introduction is a general plea for the recognition of pai-

and produces fatigue, children may be said to follow the *law of economy of effort*, which, in subordination to the law of self-protection, is the great law of psychic life in childhood.

2. In *learning language* children adopt those forms easiest to them, using instinctively gestures before words and, later, imitating the sounds of objects in onomatopæsis, which is itself a sort of oral gesture. The so-called generalizations of children and their sometimes striking association of ideas are all owing to the repugnance on their part to making the effort necessary in using new terms. To keep applying old terms to new objects is often easier than to learn new words, and hence children continue using the same word for objects, sometimes the most disparate, which however have happened in their mind to be associated in some far-fetched way.

3. In all his *conceptions* and *thinking* the child tends to economy of effort. His ideas and images are concrete, because the concrete is easier to grasp than the abstract. He repels instinctively the idea of infinity and immortality, because these require too much effort in thinking them. For the same reason he hates innovations, likes to hear the same story over and over again without the change of a single phrase, and must be put to bed with the same ceremony every night. This 'misonéism' is very serviceable to the child in helping to establish habits of routine and to give a certain settled equilibrium to his ideas.

4. The *sensibilities* and *feelings*, when excited, are still more wasteful of vital energy, and here too the children tend to conserve their strength. They are 'myopic to pain and presbyopic to pleasure.' Anything may serve them as a plaything and the most commonplace happenings of their everyday life may delight them. Their imagination turns reality into a romance and, since wishes do become horses, beggars may ride. But the joys and sorrows of childhood are only skin deep, and the caresses and jealousies of children are often only an exaggerated mimicry of the affections. The tendency of childhood, therefore, is, not to love, but to be loved; because this gives pleasure and protection; while, on the other hand, to sympathize with others and share their joys and sorrows would consume vital energy.

5. Hence the young child is scarcely susceptible to real *love* at all. That this is true is shown by the rarity of cases in which children fall in love, *e. g.*, Berlioz at eight years, Rousseau at eleven, and Marie Baskirtseff at twelve. These exceptions to the rule serve only to show what an enormous expenditure of nervous energy accom-

panies the presence of deep passion and how important it is that children should be preserved from it, as, indeed, they usually are by the very superficiality of their affectionateness.

6. Even the *moral sense* is subject to the 'law of the least effort.' A child is naturally prone to resentment, readily lies, easily becomes conceited, and thinks the whole world was made for him. In morals he is essentially an egotist, but gradually puts himself into accord with us and learns to respect the rights of others, because he receives more caresses and bonbons by so doing.

In a word, the same 'law of least effort' that governs all the phenomena of sociology and psychology necessarily governs all the phenomena of child life likewise. Before birth, the fœtus, like a parasite, draws off to itself from its hostess all the materials it needs for its growth and development, utilizing all the maternal organism, blood, respiration, and everything to its own advantage without giving anything in return. The selfishness of the young child is but a continuation of the same process of unconscious parasitic assimilation of its environment.

HERMAN T. LUKENS.

BRYN MAWR COLLEGE, PENNA.

PHYSIOLOGY AND BIOLOGY.

An American Text-book of Physiology. Edited by WILLIAM H. HOWELL. Philadelphia, W. B. Saunders, 1896. Pp. 1052. \$6.00.

A standard text-book of physiology is, perhaps, more important for the student of psychology than a handbook of psychology. Each of us must form his own apperceptive system of psychology based on data gathered from many sides, but physiology is essential to us, and here we are, to a certain extent, dependent on compilations. We have been fortunate in having such excellent works as Foster's *Text Book* and Hermann's *Handbuch*. Foster has an extraordinary insight into the essential bearings of physiological research and great ability as an expositor. For purpose of reference, however, his book is defective, because it gives no references and not sufficient facts. Hermann's *Handbuch* dates from 1879, and the intervening years have witnessed great progress in physiology. There is consequently room for a new text-book useful to the psychologist.

The work before us is called an American Text-book, which may be taken to indicate that it is written by American authors and per-

haps that it contains frequent references to research carried out in America. The coöperation of ten of our leading professors of physiology sets an example to other sciences, and the result shows that this example should be followed. With the progress of science specialization and coöperation become equally needful. There is no physiologist so competent to cover the whole field of the science as to write on a subject selected by him. The fact that there is some overlapping and some diversity of opinion as to methods and results will probably be stimulating to the student and give him a correct idea of unsolved problems and recent progress.

The introduction and the parts on secretion, chemistry of digestion and nutrition, movements of the alimentary canal, bladder and ureter, and blood and lymph are written by the editor, Professor Howell, who shows the clearness of exposition of Professor Martin, his teacher and predecessor at the Johns Hopkins University. Circulation is treated by Professor Curtis of Columbia University and Professor Porter of Harvard University; respiration and animal heat by Professor Reichert, of the University of Pennsylvania; reproduction by Professor Lee, of Columbia University, and the chemistry of the animal body by Professor Lusk, of Yale University. These sections are likely to be of special interest to the psychologist, as he is most ignorant of the subjects. The exposition is clear throughout, and the reputation of the authors is a sufficient guarantee of its accuracy.

The section on general physiology of muscle and nerve is by Professor Lombard, of the University of Michigan, and that on the central nervous system is by Professor Donaldson, of the University of Chicago. The latter occupies 139 pages, and will be found to be of great value for reference. The author treats separately the nerve cell, the groups of nerve cells and the nervous system taken as a whole. This article, as others in the book, is well illustrated by tables, curves and illustrations.

The article on vision by Professor Bowditch is, I think, the best we have, with the exception of the large works by von Helmholtz and Aubert, and these are less contemporary. Within the limits of 64 pages the more important facts of physiology and psychology are shown in excellent perspective, with due regard to the more recent advances. The psychological part of the article on hearing by Professor Sewall, of the University of Denver, does not seem to me so good. We are told that "sound, in its physiological meaning, is a sensation which is the conscious appreciation of internal changes occurring in certain cells of the cerebral cortex;" that loudness depends on 'amplitude or

the extent of motion of the air molecules,' of 'the middle C of the piano * * * representing 132 vibrations,' etc.

The editor holds that "consciousness is a property of the cortical nerve cells," as contractility is a property of muscle tissue, and that psychology is a province of physiology; but he admits that consciousness is a fact which physiology 'cannot as yet explain.' As a matter of fact, psychology is, as a rule, excluded from the book, which does not interfere with its value to the psychologist. I believe that the student of psychology should have followed a regular course in physiology, and should keep such a work as this at hand. In turn, I think that the physiologist and the physician, especially now, when ophthalmology, otology and neurology are important departments, should have followed a course in psychology, and should subsequently not neglect his handbook of psychology.

J. McKEEN CATTELL.

Charles Darwin and the Principle of Natural Selection. E. B. POULTON. Century Science Series. New York, The Macmillan Co., 1896. Pp. viii+224. \$1.25.

This is a remarkably clear, direct and modest account of the life and work of Charles Darwin, by the Oxford exponent of Natural Selection. Professor Poulton has known how to give the truest relief to the portrait of a great man, the relief which is secured by simplicity of statement and the unadorned narration of facts which are in themselves their own glory. One rises from the perusal of the narrative with a sense that science is not easy even to the man to whom it comes easiest—to the man of the industry, good judgment and ability which constitute the most normal and sane genius—and that it is inaccessible to the man to whom the secrets of nature are tools to be juggled with or stones for the building up of systems. Indeed, the two things which impress the student of the work of Darwin are, to my mind, his freedom in the use of hypotheses and the soundness of the 'judgments of value' which he passed upon the facts of nature. Professor Poulton marks both of these lessons, and they are both needed in this time when one school decries the use of imagination which constitutes the life of science, in the interest of the sort of cataloging of facts which the child in the nursery does before he begins to think, and the other shows little sanity of judgment in dealing with the value of this fact or that for the purposes of synthesis. There will always be classifiers by trade and systematizers by passion; but it is just the nature of true science that she bars the gate of

her kingdom to both of them and opens it to the man whose vision of a fact is at once also a sound judgment of its meaning and value.

Thatsächlich, the one thing on which there may be difference of opinion among those who believe in natural selection, is Professor Poulton's treatment of it as a causal or, as he says, a 'motive' principle. I say among those who believe in natural selection, for, of course, there is no gain in anticipating the criticisms of those who do not. But speaking strictly *entre nous* to the Darwinians themselves—can natural selection be spoken of in these terms? It gives the enemy cause for stumbling, for they immediately fall to asking: "How can a thing be selected before it is produced? And if it is first produced, there is your motive, your causation, already accomplished before natural selection comes in at all." I believe this position is quite invalid; Professor Poulton shows it so over again: but the general question familiar to psychologists as between the efficient and the formal cause comes to mind. The form of the result, as, for example, in Darwin's own illustration of the house as given in this book (page 116) is due to natural selection in the sense that without natural selection it would not have been what it is. Here there is no dispute among Darwinians. But the efficient or real motive principle is to be found rather in the positive forces of life which enables the creature selected to live and beget his kind, under the recondite laws which issue in continued variations; these are the positive things, and the the operation of natural selection is absolutely dependent on their continued working. Suppose, for example, they should be right who seek to prove that there is an impulse toward certain preferential lines of growth and reproduction in the life processes themselves, then natural selection would remain exactly the same principle that it is now; it would still state the conditions which limit the survival, and so the perpetuity of kinds. I think Darwinians should recognize this; for it is only when they do that they will put an end to the senseless criticism which they get in the terms of the pseudo-quotation above.

And the need of insisting upon it goes farther, since it is in this assumption that natural selectionists sometimes get their air of 'claiming the earth,' so to speak. It is clear that there is an immense amount of research ahead in the defining of the positive principles of life and development, in accounting for the quantity and distribution of variations, in ascertaining the positive qualifications which some creatures may have over and above others, whereby the former are constituted as the fittest to survive under the operation of natural selection. And each statement of a positive qualification is a real addition

to the theory of evolution, although it leave natural selection exactly where it was before, only defining its sphere of application as wider or narrower, as the case may be. None of these new determinations, it seems to me, can ever overturn natural selection, since that is a statement simply of the difference of fate which must overtake organisms as long as there are different conditions of living, differences of endowment, and different phases in the cycles of life. But just in as much as these determinations truly describe the creatures which survive, it is they, and not alone the mere ordeal which they may have survived, that is of positive value for evolution science.

In conclusion, it is of interest to note—and of very peculiar interest to psychologists to note—as Professor Poulton does, that both Darwin and Wallace declare in their correspondence, each for himself, that it was the study of Malthus on population which led to the discovery of the principle of natural selection.

J. MARK BALDWIN.

VISION.

Eine neue Theorie der Gesichtswahrnehmung. K. UEBERHORST.
Ztschr. für Psychol. u. Phys. der Sinnesorgane. XIII., 54-65.
1896.

The author proposed a new theory of visual perception in 1876, which he now perceives to be erroneous; and, in the course of years, he has composed the one which is here published and which was read before the International Congress of Psychologists at Munich last summer. He states his present theory at once, and in these terms: Visual perception, like perception in general, is neither sensation nor knowledge, but the product of a special psychic activity, whose essence consists in the binding together into a peculiar unity of a sensation given by an organ of sense with another sensation, idea, or memory image which is present to the soul at the same time, which two factors are conceived by the unconscious intelligence as signs of one and the same object. When we enter a room that is well known to us perception proceeds far more rapidly and completely than if the room is unfamiliar; this is usually explained by saying that in the former case a crowd of memory-images are called forth by the present impression and unite with it, and thus a clear idea of the content of the room is produced. This explanation is near to being the correct one, but nevertheless it does not exactly hit it off; the supposed fusion is not

what takes place, but the real process is that, first, an intelligence which is unconsciously present in us, or, in Kantian phrase, an *à priori* knowing, refers the present impression and the memory image to one and the same object; thereupon the intuition function becomes active and produces out of the two the new form, the present clear perception, as a peculiar unitary thing. The author admits that this thought will not instantly dawn upon one, but he believes that the reader will be convinced of its truth and value by the consideration of those illusions by which we see certain plane drawings as solid forms. He discusses a number of these illusions, but it seems to the reviewer that he underestimates the difficulty of leading the reader to see that his explanation differs, except in words, which represent fictions, from the explanation usually given. All this, he says, after describing a number of common illusions, is a union effected by the Unconscious Intelligence between the present sensation and the idea which is in the mind; but it is difficult to see that there is anything in the instances chosen which throws light upon the question at issue—whether the fusion is effected by the Unconscious Intelligence, or whether it just takes place, without the aid of that mythical creation. The idea that there is something in the mind which does everything is not so much in favor among the psychologists as it was once, and merely stating it as a belief is not enough to carry conviction. Nor does it seem well-advised to devote a good portion of a paper to a diagram for showing that two points which are, to a single eye, in the same direction when looked at directly are not so when looked at peripherally—a point which most text-books (that of Norris and Oliver, to mention the latest) are content to dispose of in a line; moreover, the difference is so slight that it can only be effective for points which are very far removed from each other, and it is certainly of no moment in determining the solidity of ordinary objects, and cannot therefore furnish the complete basis for our notion of the third dimension. In conclusion, the author affirms that, since Hartmann's *Philosophie des Unbewussten*, no one has any right to doubt the existence of the Unconscious Intelligence, and hence that no one can find it unjustified if he seeks to take cognizance of its activity in the production of the perception.

Ueber Erythroopsie. ERNST FUCHS. Archiv für Ophthalmologie, xlii. (4), 207-292.

This is an extremely careful piece of experimenting and also of reasoning, and, on account of its connection with recent theory, it is worth while to report it at some length.

It has been known for some time that persons who have been operated upon for cataract frequently see things in a red light; this happens after an injury which permanently increases the size of the pupil, or after the loss of the lens, and particularly when both defects occur together. These are circumstances which predispose to true, typical erythropsia, but the immediate cause is exposure of the eyes to a dazzling light; sunlight reflected from large fields of snow is sufficient to cause it even without these favoring circumstances, but much more so if the snow fields are at a high altitude. The red color is particularly noticeable upon entering a hut after some hours of mountain climbing among the Alps, and it was upon such an occasion that it first attracted the author's attention. Its extreme brightness at this time was, doubtless, owing to an unusual amount of ultra-violet rays in the light reflected from the snow, for severe sun-burn was also experienced, and sun-burn has been shown by Widmark to be due to the ultra-violet rays. In spite of the certainty of its occurrence, this erythropsia of the normal eye has received hardly any mention in ophthalmic literature.

Fuchs' experiments were conducted partly on a mountain near Vienna and partly after widening the pupil of the eye by homatropin, in both cases by means of looking for a rather long time at bright snow, which does not need, however, to be of a blinding brightness. They were confirmed by several observers, some of whom were able to obtain the effect without either of the preliminary steps. It was found convenient, upon entering the moderately darkened room, to look upon a chessboard of alternate white and black squares. At the first instant one sees nothing, then there is a brief period of seeing green, and then the red color appears, upon the bright squares much sooner than upon the dark, to remain for three or, at the most, four minutes. With some persons the red is preceded and followed by a flash of orange and citron-green. But within a region of from three to five degrees at the center of the field the phenomenon is entirely wanting (except with certain persons upon one or two occasions). Peripherally, the red color does not reach to the limit of vision, but it does extend beyond the usual field for red. In color-tone, it is complementary to a slightly yellowish green. Looking at the snow through variously colored glasses did not in the least change the color of the erythropsia, but with glass of the same color the phenomenon did not occur at all.

Fuchs proceeds to discuss the cause of the phenomenon. The color of daylight is reddish, and that of snow-light approaches violet;

the green that precedes the red may, therefore, be the after-image of this color, though that is not quite certain. The erythropsia itself is certainly not an after-image, either positive or negative, of the ordinary kind. It bears many resemblances, however, to what Fuchs calls the after-effect of dazzling, concerning which he offers a large number of new observations, but it also differs distinctly from this. He comes to the rather doubting conclusion that it is the entoptic vision of the visual purple, but his readers will certainly feel that this hypothesis has everything in its favor. The subjective color is exactly the same as that of the so-called 'visual purple.' [It is, of course, not *purple* at all, in English, but crimson; *purple* is a piece of shockingly bad translation; nor is it by any means made out that the substance is a visual substance. I am glad to see that my name for it, rod pigment, is becoming accepted.] Usually the color is invisible to us, like any color which is unchanging and which covers the whole field of view; but after it has been thoroughly bleached out the eye is sensitive to its sudden re-appearance. Even though the rods do not perceive color, it would be sufficiently reflected from them upon the cones to become visible, and there may easily be enough of the substance in the pigment of the epithelium to account for the rare cases in which it is seen in the fovea. That red glass prevents its occurrence corresponds with the fact that red light does not bleach out the rod pigment; and that it is most vivid after blinding through green glass is in accordance with the fact that the maximum absorption of the rod pigment is in green. Ewald, Tait and Boll believe that they perceived the rod pigment entoptically on first waking up in the morning, and by these painstaking experiments of Fuchs their supposition is made probable beyond a reasonable doubt.

CHRISTINE LADD FRANKLIN.

BALTIMORE.

PATHOLOGICAL.

Manuel Pratique des Méthodes d'Enseignement spéciales aux Enfants Anormaux (Sourds-muets, aveugles, idiots, bégues, etc.).

HAMON DU FOUGERAY et L. COUËTOUX. Préface du DR. BOURNEVILLE. Paris, Progrès Médical, 1896. Pp. 288.

This volume appeals to such physicians, teachers and professors as are especially interested in the questions of the care and education of certain classes of defective children. The book is divided into five parts, the first of which is concerned with deaf-mutes, the second with

the blind, the third with idiots, the fourth with stammerers and stutters, and the last with individuals presenting combined anomalies, such as the deaf-mute-blind, the deaf-mute-idiotic, the blind-idiotic and the deaf-mute-blind-idiotic. Under each heading we are given an historical account of the condition described, statistics of the disorder, details as to institutions existing in France for the care and treatment of these classes of cases, a summary of the legislation relating to them and a résumé of the methods of pedagogic treatment employed.

In the instruction of deaf-mutes the authors, after presenting the arguments *pro* and *con*, express themselves wholly in favor of the pure oral method of inculcating language and opposed to the mimetic method or mixed oral and mimetic method. The chapter describing the pure oral method is based on the work of Goguillot. This method, in brief, consists of a preparatory course of education of the sight, touch and muscular sense, followed by touch-studies of the action of the respiratory muscles, then of the lips, tongue, buccal cavity and larynx. After this comes the emission of sound, and by proper placing of the muscles of articulation, under direction of the teacher and with the help of a mirror, the pupil acquires first the words, then the consonants, and finally the combinations in syllables and words.

The chapter on the instruction of idiots is simply a reproduction of the writings of Bourneville upon that subject, the methods employed at Bicêtre and fully described in the various reports issued from that institution during the past sixteen years.

For stammering and allied disorders of speech the authors recommend and detail, at some length, the Chervin method of treatment. Chervin began to apply his method in 1846 and published a book upon the subject in 1895. The method does not differ essentially from the rational physiological systems in vogue all over the world at the present day.

Récherches cliniques et thérapeutiques sur l'Épilepsie, l'Hystérie et l'Idiotie, compte rendu du service des enfants, idiots, épileptiques et arriérés de Bicêtre pendant l'année 1895. Par BOURNEVILLE, médecin de Bicêtre, avec la collaboration de MM. Boncourt, Comte, Dardel, Dubarry, Leriche, Lombard, J. Noir, Pilliet, Ruel, Sollier, Tissier. Vol. XVI., avec 31 figures dans le texte et 8 planches. Paris, Progrès Médical, 1896. Pp. 254.

This is the sixteenth volume of the famous studies made by Bourneville and his assistants at the several hospitals united under the name Bicêtre. These annual reports are a rich field for the searcher

after facts in the domains of pathology, therapeutics and pedagogy as applied to epilepsy, hysteria and idiocy. The first part of the volume for 1895, as in former years, is devoted to a history of the service during the year, and in this we note the creation of special classes for the feeble-minded and a chapter on the medico-pedagogic treatment of abnormal children. The second part consists of clinical and pathologico-anatomical studies of thirteen cases of idiocy and epilepsy, containing a valuable addition to our repository of facts relating to these subjects. The third part gives us the result of observations upon the effects of certain remedies. Three cases of cretinism treated by extract of the thyroid gland exhibited marked improvement in the intellectual sphere as well as nutritive changes, such as loss of weight and increase in height. In regard to the matter of nutrition, Bourneville shows in a number of other cases the value of the thyroid juice in diminishing obesity. A chapter on the exhibition of bromide of camphor in vertiginous epilepsy demonstrates its great utility in that form of the disorder.

FREDERICK PETERSON.

NEW YORK.

Grundriss der Psychiatrie in klinischen Vorlesungen. Theil I.: Psycho-physiologische Einleitung. Theil II.: Die paranoischen Zustände. C. WERNICKE. Leipzig, Thieme, 1894-6. 8°. Pp. 178.

Drily written and, though clear, not easy reading, Professor Wernicke's book, short as it is, is already the weightiest of the attempts, of which several have lately been made, to apply psychological laws to the unravelling of what happens in disordered mental function. Part I. is a synopsis, many pages of which deserve to become classic types of exposition, of that modern scheme of cerebro-mental action of which Wernicke by his little work on Aphasia was himself one of the founders. The great lucidity of the statement now made shows us once more how surely protracted meditation on a subject makes a man its master. For psychiatric purposes the chief result of this Part is that the insanities (being diseases of the cortex, which is the organ of association) should psychologically all be explicable as disorders, defects, excesses or perversions, as the case may be, of the associative function.

In Part II. the author applies this notion to delusional conditions, his account of which is entirely unconventional and reveals the man of original perceptions on every page. For most of his new distinctions

and classifications—unfortunately all with Greek names—the reader must consult the original. I will confine myself to a brief notice of the most important thing in the book, which is the explanation, by a single underlying cause, of the whole complex of delusional symptoms. How is it possible to find such a mass of false ideas at war with each other and with reality, such a loss of the sense of probability, such hallucinations, such inter-current emotional states and motor tendencies, in one patient? Professor Wernicke answers by what he calls his *hypothesis of sejunction* or dissociation. A pathological process has loosened the firmly connected system of associations, so that a large number of those originally there have become impossible, and a ‘disintegration of the personality’ results. Confusion in perception and dementia are evidently nothing but gaps in normal association, replaced or not by associations that are non-normal. But our author explains the phenomena of excess, the impressed thoughts and hallucinations by his theory, as well as the phenomena of defect. The two run together, excess in the processes that stand over being the consequence of the loss of such other processes as may have disappeared. Defect is thus primary and excess secondary, as in the ordinary theory of dreaming and the theory by which the present reviewer¹ and Mr. Parish² have treated illusions and hallucinations. Professor Wernicke applies the theory of sejunction very ingeniously to a large number of symptoms, sensorial, ideational and motor, and of course dilates at length on delusions as explanatory theories by the patient of his elementary disturbances. The false connection with himself (*Beziehungswahn*) which the patient finds in so many different experiences, the distortions of memory, the distinction between presently active and residual morbid processes, all come in turn to be suggestively discussed.

I cannot help thinking, for my own part, that the explanation of irritative phenomena (or *Reizerscheinungen*) by defects of association (or *Ausfallerscheinungen*) has to bear somewhat too heavy a burden in Professor Wernicke’s pages. If the mere stoppage of association-paths be by itself enough to heighten any process at which the stoppage may occur, then, whenever we hesitate for a word, we ought to get the last cue-word in the shape of an hallucination of hearing—of what W. calls a *phonem*, rather than as a mere verbal *idea*. But I can discover no tendency to such sensorial vivacity in the last idea reached in such cases, and this fact, I must confess, has given me

¹ Principles of Psychology, II., 122 ff.

² Ueber die Trugwahrnehmung, München, 1894, p. 105 ff.

some uneasiness about the theory of hallucination suggested in my own book. It seems now time for the 'discriminating stage' of criticism to be applied to that theory, and of course the field of paranoia presents itself as the place *par excellence* for working the discriminations out. The great lucidity and rationality of many paranoiacs, their freedom from any speech disturbances or other *Heerderscheinungen* in the way of directly perceivable defect are hard to reconcile with the view that their 'false voices' (which would seem to be *Heerderscheinungen* of excess) are secondary rather than primary symptoms. In many important respects there are analogies between patients with delusions and cases of hysteria such as those that Janet, Brewer and Freud have explored, and this would suggest that it might be well to search for parasitic systems of subconscious ideas as a possible source of some of the trouble in the former cases. In one way ('disintegration of personality') Wernicke's 'sejunction' formula coincides with Janet's, yet Wernicke ignores altogether the notion of subconscious ideas; and indeed it is evident that if they exist we need quite new methods of finding them out. But be all this as it may, it is still certain that Wernicke's hypothesis of sejunction or dissociation opens a new era of interpretation in mental pathology and gives to all observers of the insane a new task in the way of something definite to verify, complete or refute. This is a great service and the book that has performed it ought to be translated without loss of time.

W. J.

NEW BOOKS.

- Psychologie der Naturvölker. Ethnographische Parallelen.* JACOB ROBINSOHN. Leipzig, Friedrich, no date. Pp. 176.
- Lehrbuch der Psychologie.* F. JODL. Stuttgart, Cotta'sche Buchhandlung, 1896. Pp. xxiv+767. M. 12.
- Outlines of Psychology.* W. WUNDT. Trans. by C. H. JUDD. Leipzig, Engelmann; New York, Stechert, 1897. Pp. xviii+342. \$1.75.
- Theorie der Begabung.* R. BAERWALD. Leipzig, Reisland, 1896. Pp. x+289. M. 5.
- Das konträre Geschlechtsgefühl.* H. ELLIS and J. A. SYMONDS. Bibliothek der Socialwissenschaft. Ed. by H. KURELLA, No. 7 (original Ausgabe). Leipzig, H. Wigand, 1896. Pp. xi+308.

- Atlas of Nerve Cells.* M. A. STARR. With the coöperation of OLIVER S. STRONG and EDWARD LEAMING. New York, The Macmillan Co., 1896. LIII. plates, 13 diagrams. Pp. 79, 4to. \$10.00
- Das Ideal des 'ewigen Friedens.'* LUDWIG STEIN. Berlin, Reimer, 1896. Pp. 65. M. 1.20.
- Die Freiheitslehre bei Kant und Schopenhauer.* D. NEUMARK. Hamburg and Leipzig, Voss, 1896. Pp. xii+89. M. 2.
- Die Autonomie der Moral.* K. B. R. AARS. Hamburg and Leipzig, 1896. Pp. 121. M. 3.
- Die Psychologie in der Religionswissenschaft.* E. KOCH. Freiburg and Leipzig, Mohr, 1896. M. 2.80.
- The Cell in Development and Inheritance.* E. B. WILSON. Columbia Univ. Biolog. Series, IV. New York and London, The Macmillan Co., 1896. \$3.
- Dritter internationaler Congress für Psychologie in München* (Aug. 4-7, 1896). München, Lehmann, 1897. Pp. xlv+490.
- Studien zu Methodenlehre und Erkenntnisskritik.* F. DREYER. Leipzig, Engelmann, 1895. Pp. xiii+223. M. 4.
- Addresses and Proceedings of the National Educational Association, Buffalo, N. Y.* Published by the Assoc., Chicago University Press, 1896. Pp. viii+1088. \$2.
- Manual of Logic.* J. WELTON. University Tutorial Series. Vol. I. Deductive, 2d ed., revised. Vol. II. Inductive. London, W. B. Clive; New York, Hinds and Noble, 1896. Pp. xxii+411 and xiii+292.
- Manual of Ethics.* J. S. MACKENSIE. University Tutorial Series. 2d ed. London, W. B. Clive; New York, Hinds and Noble, no date. Pp. xxx+355.
- Matière et Mémoire; essai sur la relation du corps à l'esprit.* H. BERGSON. Paris, Alcan, 1896. Pp. iii+279. Fr. 5.
- Charles Darwin and the Theory of Natural Selection.* E. B. POULTON. Century Science Series. New York, The Macmillan Co., 1896. Pp. viii+224. \$1.25.
- Christianity and Idealism.* JOHN WATSON. Publications of the Philosophical Union of the University of California. Edited by G. H. HOWISON. Vol. II. New York, The Macmillan Co., 1897. Pp. xxxviii+216. \$1.25.
- Problems of Biology.* GEORGE SANDEMAN. London, Sonnenschein; New York, The Macmillan Co., 1896. Pp. 213. \$2.

AN EXPLANATION.

My attention has been called to the fact that a meaning, far other than any intended, might be put in certain words of mine, in my estimate of Mr. Sterrett's book in the January issue of the REVIEW. It is suggested that a reader might take my words (p. 78) to mean that Mr. Sterrett's results were not reached independently and that he had used the work of recent investigators without giving them due credit.

I wish to prevent any such misinterpretation by saying that when I wrote, I had no such thought in mind and that I have good reason for knowing that Mr. Sterrett's results are the outcome of his own insight and independent reflection. In pointing out the agreement of his thought with that of the authors mentioned in my review, the real intention was to emphasize the healthiness of his originality.

ROGER BRUCE JOHNSON.

MIAMI UNIVERSITY.

NOTES.

DR. JAMES WARD has been appointed to the new Professorship of Mental Philosophy and Logic in Cambridge University.

WE regret to record the death of Professor W. Wallace, Professor of Moral Philosophy in the University of Oxford, who was killed on February 19th by a fall from a bicycle.

WITH the current issue the *Vierteljahrsschrift für wissenschaftliche Philosophie* begins a new volume (XXI.), with a somewhat enlarged programme, under the editorship of Fr. Carstenjen and O. Krebs, to whom the editorial care had been transferred by Avenarius before his death.

THE same issue of the *Vierteljahrsschrift* announces a prize (of 500 M.) for the best essay on the subject: *Nachweis der metaphysisch-animistischen Elemente in dem Satz der Erhaltung der Energie und Vorschlag zur Ausschaltung dieser Elemente*. The length is to be 3 to 4 forms of the *Vierteljahrsschrift*, the language German, and the limit of time October 1, 1897. The essays may be sent to either of the editors, to Professor E. Mach, of Vienna, or to Professor A. Riehl, of Kiel.

THE firm of Ruether u. Reichard, of Berlin, announce a *Sammlung von Abhandlungen aus dem Gebiete der pädagogischen Psy-*

chologie und Physiologie, to be issued in complete essays, 6 to 8 per year. The series is to be edited by Professor H. Schiller, of Giessen, and Professor Th. Ziehen, of Jena.

WITH the number of December, 1896, the *Revue Philosophique* issues its second general Index (1888 to 1895, pp. 91, Fr. 3), prepared by J. Clavière. As it contains an analytical table of matter, as well as an index of names, it will be serviceable as an index to the important literature of that period.

WE have received the annual 'Addresses and Proceedings of the National Educational Association' for 1896 (University of Chicago Press), and the 'Report of the Commissioner of Education' for 1893-4 (2 vols.), and 1894-5 (2 vols.); Washington, Government Printing Office, 1896; both valuable repositories of information for students of education.

PROFESSOR JAMES SETH has been added to the editorial staff of the *Philosophical Review*.

L'Anthropologie states that a government School of Science has been established at Madrid with twenty-seven professorships, one of them being a chair in physiological psychology to be occupied by Professor Simmara.

WE may call attention to the full description (with cuts) by Professor Mosso, of his new Myotonometer (for studying the tonicity of the muscles in man), in the *Arch. Ital. de Biologie*, XXV., fasc. 3, 1896.

THE *Psychological Index* for 1896 will be issued about the middle of March. The arrangement whereby it is also published in the *Année Psychologique* is continued, and the *Index* will hereafter have the coöperation of M. N. Vaschide, of the Sorbonne, in the preparation of the French titles. We are glad also to announce that, by coöperation between the REVIEW and the *Zeitschrift für Psychologie u. Physiologie der Sinnesorgane*, an interchange of titles between the English and German bibliographies has been arranged, so that, beginning with the *Index* for 1896, the two will be in these respects substantially alike. A difference in the number of titles in favor of the German bibliography will, however, still be probable in view of its later date of publication.

This issue of the REVIEW is enlarged to accommodate the Proceedings of the American Psychological Association.

